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ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

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AND

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ERRATA.

Vol. V. Nos. 15 and 16.

Page 346, line 7, *for* " diminished " *read* " direct."
Page 353, line 22, *for* " radial " *read* " radiant."
Page 384, line 8, *for* " K " *read* " lightning."
Page 385, line 8, *for* " can " *read* " cannot."
Page 385, line 25, *for* " pressure " *read* " presence."
Page 387, line 22, *for* " forced " *read* " fused."

VOL. V. 1876. No. 13.

The Forty-first Ordinary General Meeting was held on Wednesday, the 12th January, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

At the commencement of the proceedings the Chair was taken by Mr. LATIMER CLARK, who rose and said:—

GENTLEMEN, — I have now the pleasure to resign my late functions for the past year, and to vacate this Chair in favour of Mr. C. V. Walker, our new President.

The Chair having been taken by the President—

Mr. W. H. PREECE said: It is my pleasure to-night to have to propose to the Society a vote of thanks to our retiring President, Mr. Latimer Clark, for the uniform attention he has paid to his duties during the past year. I have especial pleasure in doing this, because it is now twenty-five years ago since I first entered the field of telegraphy as Mr. Latimer Clark's assistant in his office, and many a time and oft I have spent days and nights travelling over lines in such weather as this, "sitting on rocks and musing o'er flood and fell" with Mr. Clark, watching for the vagaries of electricity which then were developing themselves to us for the first time; and never shall I forget the pleasure of those days in searching in a new field for novel effects and new ideas in company with one whose mind was so well qualified to instruct and direct my more youthful mind. The Society itself has no doubt gained enor-

mous credit throughout the whole world through having at its head a man of such eminence as Mr. Latimer Clark, and his wide-spread reputation has done much to increase the popularity of our proceedings and especially of our journal. His connection with telegraphy can never be forgotten, for there is scarcely a branch of it, whether on land or submarine lines, which is not in some way associated with the name of Clark, either by the improvements effected by his brother, or by the greater improvements he has himself introduced. Nor can we forget the attention and constant attendance which he has given to our meetings, and the care with which he has presided over our discussions. No one can forget his own admirable address upon the loss of our greatest member during the past year, Sir Charles Wheatstone. It is one of the few papers which will live for years in our endearing recollection. Again, we cannot forget the hospitality which we have received at his hands, for certainly the *soirée* at Willis's Rooms was one of the most successful, most pleasurable, and most delightful we have had. I am therefore quite sure you will all cordially agree with me in conveying our thanks to Mr. Latimer Clark for his great attention to the duties of his office during the last twelvemonths.

MAJOR WEBBER, R.E.: Mr. Preece has left me very little to say. He has touched upon all those points with which you are so well acquainted in regard to Mr. Clark's connection, not only with this Society but with the history of telegraphy, therefore I can only follow him up by seconding his proposition, that a vote of thanks be earnestly accorded to our past-President, not only for his assistance in the formation of this Society in its early days, and his constant attendance at the meetings, but also for the hospitality which he showed on a late occasion in entertaining the members of the Society and their friends and so large a number of persons connected with the scientific world. I beg to second Mr. Preece's motion, and to ask you to accord to Mr. Latimer Clark that which he has so well earned.

THE PRESIDENT: It has been proposed by Mr. Preece, and seconded by Major Webber, that the best thanks of the Society be given to Mr. Latimer Clark for his able conduct in the chair during the Session which is now ended. Nothing I could say

would add to what you have heard, and I am quite sure you will give your best thanks with acclamation to Mr. Latimer Clark.

The motion having been passed by acclamation,

Mr. LATIMER CLARK said: I am much touched by the manner in which this motion has been proposed by Mr. Preece and received by the meeting. It comes with especial grace from him for the reasons he has stated. Throughout almost the whole of my long connection with telegraphy I have had the pleasure of knowing Mr. Preece intimately, and I can say no more than that I feel deeply the way in which he has put and you have received the motion. I can only add that my connection with this Society has been to me the highest source of pleasure and gratification; my attendance here has been always a labour of love, and I hope I may long be spared to render what assistance I can in the promotion of the interests of the Society.

The President then proceeded to read his inaugural address.

GENTLEMEN,

In accordance with custom, which in practice has become law, it is my duty to address you before occupying this Chair; but I have yet another duty to perform—one which has a prior claim upon me—to thank you for having placed me in this high position.

It is on record, and there is much truth in it, that some men are born to honours; some men achieve honours, and some men have honours thrust upon them. The last is my case. You have raised me to a position to which I, of all men, would have been the last to aspire, having done so little, having been unable to do anything but so very little, for the Society of Telegraph Engineers since you honoured me with a seat at the Council Board. You have withdrawn me from comparative retirement to occupy this Chair—to preside over the meetings of a body of men among whom are many with far higher claims to preside over me than I over them. And you have raised as it were an outsider to this high position, one, that is, whose lot was cast outside the limits of the great Telegraph Companies, having for many years controlled and watched over the interests of a little—by comparison—a little self-contained telegraph kingdom—namely, that on the South Eastern Railway.

A sketch of the telegraph system on this railway may be a useful contribution to the history of telegraphs in this country. There are some special features about it which may be known to many members of this Society, but which are not generally known. This system ante-dates the formation, if not the conception even, of the "Electric Telegraph Company." The original agreement between this Railway Company and Mr. (now Sir William Fothergill) Cooke, is dated the 11th of September, 1845; but construction was begun some months in anticipation of this date. The terms of the agreement were, that for the main line, between London and Dover, a four-wire telegraph should be constructed for £225 per railway mile, inclusive of instruments and batteries; and that for branch lines a three-wire telegraph should be erected, at a cost of £175 per railway mile, exclusive of instruments and batteries: the contractor at the same time undertook to keep the lines of telegraph, originally erected under this agreement, in good and efficient repair for one year after the completion of the first half of the main line. The Telegraph Engineers of 1876 will be inclined to hold their breath at hearing of the price paid in 1845, or thirty-one years ago, and to regard the telegraph in those days as a rather costly luxury; and so it was. But if one of the then Railways, when the iron road was barely in its teens, and traffic was comparatively so small, could boldly venture to make a purchase such as I have described, what excuse can any Railways of to-day have, if they fail to avail themselves of all the advantages to be derived from electricity? the more so, as the benefits are in these days to be acquired at a cost—by comparison—so moderate? But large though the outlay was, and larger still as it has become with the extension of the Railway and the creation of new necessities, no better investment could have been made. Under the terms of the agreement, the Telegraphs erected became the absolute property of the Railway Company, as much so as are the locomotives, the carriages, or the rails. And so they remain to this day. They were erected essentially for railway purposes, and with no ulterior object in view; and, having been purchased at a price that could be felt, it is no matter of surprise that the Company were far more disposed to hold them against all comers than to part with them.

Hence, notwithstanding the several overtures that were from time to time made, they still remain, as I have said, the absolute property of the Railway Company. But the public were not on this account excluded from them; for in the first instance they were opened free under special circumstances, and within certain limits. Shilling messages had been established at an earlier date on the Great Western Railway, between Paddington and Slough. How soon after 1846, June 18, the date of the Act, the Electric Telegraph Company opened to the public, I cannot remember, but on August 15, 1846, the following circular, bearing my signature, was issued by authority on the South Eastern Railway:—

"I find that many private messages are received for gentlemen to send to different parts, either for letters, small parcels, or things of that sort. I beg to say that the Directors do not allow this use to be made of the telegraph, and that it is to be used entirely for the Company's service, except in extreme cases, where kindness or humanity would dictate, such as illness, death, &c., and for the public press at Folkestone, and then only to a limited extent."

These privileges, however, lasted for a few days only; for on the first day of the following month rates were issued. The scale was based upon three-halfpence per mile for twenty words, the minimum being five shillings. The rate from London to Dover was 11*s.*, to Ramsgate 12*s.* 6*d.*

I well remember a remark that was made at this time, and it came from no mean authority, that it would not do to make the telegraph rates too low for fear of reducing the traffic receipts on the railway, by inducing passengers to use the wire instead of the train; and hence a Parliamentary fare and a half, or three halfpence per mile, was adopted. From that date until February 5, 1870, when the goodwill was transferred to the Post Office, these railway telegraphs were open to the public as feeders and distributors for the Telegraph Companies proper. And now, as agents for the Post Office, the Railway Company keep offices open, and deal with messages on commission.

Many postal telegraph offices having been established in parts of Berkshire, Kent, Surrey, and Sussex, served by this line, and to the relief of the railway wires, and as the Post Office were in

urgent want of wires, the Company were able to spare and (as the property was their own, it was in their power) to sell them some three or four hundred miles.

Meanwhile, the service of the Railway and the comfort and safety of the travelling public were the prime objects of consideration, and were always kept prominently in view. "Onward" is the motto of this Company; and true to their motto have they proved themselves. This was the more easy from their being unfettered from without. They were able to turn their freedom to good account. What time a new want arose, they could meet it from their own resources. In no matter have they taken a more prominent position, and one more in advance of many or most railways, than in "train-signalling." They began this very early, long before instruments specially adapted to the work were constructed. The speaking instruments, double or single needle, were used, and almost without exception were the same that were also employed for service and for public messages. Even in those days, when trains were so few, this was a case of working under difficulties. The original circular of instructions, dealing with the telegraphs generally, and train-signalling especially, that bears my signature, was issued in 1846, February 16. It ordered that "The arrival and departure of every train is to be telegraphed to the next *up* and the next *down* stations;" or in other words, that the speaking instruments (which for message purposes are in groups) were to be used for ordinary train-signalling, which work, as I need hardly say, was done but indifferently, even when trains were as at that time few and far between. But in course of time, as traffic increased, the cry was for more electricity, and yet more; and the important step was taken in January 1852 of erecting a separate wire and constructing a new class of instruments specially adapted for "train-signalling" alone, and available for nothing else.

There were so many trains to talk about, and so much to say about each, and to say it on the instant, that there was neither time to spell words nor to read them if sent, even had there been hands at liberty to send, and eyes to see and read, which there were not.

The ear alone was at liberty, and the new instruments new

provided spoke to the ear, and said all that had to be said by a few simple sounds on single-stroke bells. This was followed by the issue, on March the 1st, 1852, of books in which were registered the signals made and the time of making them, not only as a permanent record for future reference, but as an *aide-mémoire* to the signalmen themselves.

In due course these sound-signals on their way from station to station were made to enter every gate-house at level-crossings, for the information of the gatekeeper; and were also led onward to the platforms, so that the station staff might know where the trains were.

On January 30, 1860, just fifteen years ago, and before the unhappy expression "block system" was well known, the special wire was completed and the new instruments connected up throughout the whole of this Railway, and every inch of the line has been worked from this date on the "block system" proper.

While this was in progress, roads as well as trains were increasing in number, and each road required its wire and pair of instruments. Reference to the register book as an *aide-mémoire* becoming more or less impracticable, an addition was made to the sound-instruments, of such a character that, while they still addressed the ear, and the message was on its way to the ear, it left its mark visible on the face of the instrument, which mark was retained there until the cause for its exhibition had passed away, showing at a glance at any moment the then condition of every road. More recently the variety of trains on the same road has so increased that the bells have been supplemented by another instrument that points to the name itself of the train that is coming; and so on. On the first of the present month, 865 instruments, including repeaters, were in use for the "block system" on 328 miles of this Railway.

I should add that the Railway Company established a telegraph staff of their own from the very first, and themselves carried out construction and maintenance; always moving "onward." If not pioneers at all points, they seem to have been in many; at least they were no laggards. At any rate they began early to prove

that a Railway Company, in its own internal strength, could with credit maintain its position as its own Director and Manager of Telegraphs.

They have also done much work beyond their own wants, and also outside their own gates, having erected and maintained telegraph lines on their own property for the Electric and for the Magnetic Telegraph Companies and for the Post Office, as well as lines for the Post Office on the public roads, and postal telegraph-offices in towns and villages. Some ten years ago they introduced electricity into their trains, and have already fitted up about 1,500 vehicles, and are still going "onward" in this work.

More than twenty-three years ago, in co-operation with the Astronomer Royal, they were the first to inaugurate the distribution of time by telegraph.

More than eighteen years have passed since they went outside their own gates with clocks and watches for repairs. They have in all about 800. Their own people, the telegraph staff, clean and repair them; besides having made some fifty of the clocks. Are they alone, or almost alone, in this? The author of "Self-help," Mr. Samuel Smiles, was for some years Secretary to the Company. I think he would agree with me that the principle of self-help, which he so strongly advocates, has not been badly carried out, at least in telegraphy, on the railway to which he was attached.

The substance of much that I have been saying is that railways owe a deep debt of gratitude to electric telegraphs. But the debt is not all on one side. The following remarks on both sides of the question were written in March 1850:—

"The electric telegraph is greatly indebted to railways if not for its existence at least for the friendly hand they have held out to it, and for the protecting care with which they have guarded it; indeed the invention would long have remained immatured and void of practical existence had not the railway prepared for it a pathway from place to place, along which its capabilities could be tested. Nor has the child been ungrateful to its foster-father; it has made a tenfold return for all the protection that has been extended to it. The quiet poles and silent wires, the zinc and the

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vitriol, the brass, the ivory, the earthenware, and the gutta-percha, are far more concerned in the working of a railway than the proprietors may suppose."*

The *ten-fold* return of those days may well read a *hundred-fold* to-day: that is, after the lapse of a quarter of a century. The telegraph expenses on a railway form but a small item of the general expenses, while an efficient telegraph system contributes largely, much more largely than is universally acknowledged, to the great success of railway enterprise.

I have mentioned the protecting care with which railways had guarded lines of telegraph. One's ideas naturally go back to Paddington and Slough on the Great Western Railway. Within the fences and under the protecting care of that railway the then young and feeble fledgeling—shall I say—was safely housed, and had the grand opportunity of proving its capabilities to the world at large, when it received and sent that well-known message in which was the word beginning with a letter not provided for in the code. This little line of telegraph was then one of the sights of London. Well do I remember in 1845 paying my shilling to see it. It was made known by handbills to passers-by. Here are two of them cut from my "Scrap-book," from which I will read.

[Fac-similes of these, prepared by the photo-lithographic process, are here inserted.]

Railways thus early and long afterwards were accepted as the natural place for telegraph lines. Exposure in the open country or on public roads was not thought of. Mr. Wm. Hatcher, the engineer under Mr. Cooke, who erected the London-Dover telegraphs, wrote on August 1st, 1847: "Localities where the suspension of wires is impracticable, as in streets, towns, public roads."† The British Telegraph Company were the first to prove on a large scale the fallacy of this. Contrast this fear of 1847 with the confidence, the result of experience, expressed in the following remarks made to Mr. Scudamore in 1868: "I am of opinion that the public roads may be more largely pressed into the service of the telegraph than has heretofore been the case. . . . Nothing can be better for a

* Walker's Electric Telegraph Manipulation, p. 83, § 111.

† Engineer and Contractor's Pocket Book for 1847-8, p. 380.

line of telegraph solidly erected than a good straight country road tolerably free from trees. . . . The wires in the open are not exposed to pernicious vapours and will have a very long life, and in passing villages they are not exposed to such destructive agencies as when near passing trains or railway works; nor do I think they are more liable to interruptions." * And so on.

The open country was considered equally unsafe with public roads, &c., and no one ventured, as now, to carry railway wires over, in place of through, tunnels. Open wires were hung in the tunnels, with what effect on the signals you may well imagine. In the year of Revolutions, 1848, the telegraph signals, that conveyed the exciting continental news from the coast to London Bridge Station, were so weak that none but a very practised eye could see the slightest sign of motion in the long needles then in use, which led the then Chairman to ask the clerk whether he *was* actually reading off the words he was dictating to the writer.

At this time, when so much wanted, gutta-percha began to be known. The first wires to which it was applied here were No. 8 galvanised open wires, on the Dover viaduct, in November 1848. Beginning with December 1848, and ending with July 31, 1849, wires insulated with gutta-percha were *suspended* in all tunnels between London and Dover. Meanwhile the Company had made another movement in the "onward" direction. Two miles of the above wire were joined up, wound on a drum, and taken to Folkestone, and connected up with an open wire, that was led into a single-needle instrument at London Bridge Station. The drum was put into a small boat and rowed out to sea, the wire being at the same time paid out. The end was taken to one of the Company's steamships, on board of which a large party of visitors was assembled, and there connected up with a single-needle on the London wire. At 12·49 p.m., January 10, 1849, the following message was sent direct to London through the two miles of submarine and 83 miles of land line:—Mr. Walker to Chairman,—"I am on board the *Princess Clementine*. I am successful."† Was this the *first* submarine message, or the first sent direct to London?

* Charles V. Walker, Dec. 29, 1868, in reply to Mr. Scudamore, Sept. 23, 1868.

† Walker's Electric Telegraph Manipulation, p. 104, § 103.

Before leaving the South-Eastern Railway I must say one word on the quality of the material and the high character of the work handed over by Mr. Cooke. It was simply beyond praise. The wire was best charcoal, galvanised in the best way. It is no matter of surprise that most of this in the open country remains *in situ* to this day, and we have no better wire to handle. It is a matter, too, of agreeable surprise that many of the poles of 1845 are still standing. I have lying before my office window a pole marked "original;" it is really as sound, so to speak, as ever. It is one of a few taken down for alterations some months since. The poles were of seasoned foreign timber, tapered, chamfered, Burnettized, and painted. Some of the original D. N. pillar instruments are still in use, while many of later dates from other sources are worn out.

As Mr. Hatcher was on the eve of completing his work, Mr. Cooke introduced me to the office I still hold, namely, in October 1845, just thirty and a quarter years ago. Looking back to this distant date, I cannot help thinking that I must be the oldest Telegraph Engineer in England,—at least the one longest in tenure of the self-same office,—the one who has had sole charge as Resident Engineer of the same working system of Electric Telegraphs for the longest term of years. I may well believe that to my having taken office thus early, and having held this self-same office so long and so tenaciously, is due, more than to any personal fitness on my part, the honour you have conferred upon me.

With the names of Siemens, Scudamore, Thomson, and Latimer Clark haunting me, I take this Chair with feelings of anxiety. The deep science on the one hand, and the administrative capacity on the other, which the sound of these names evokes, convinces me that I shall have to claim, and I am sure shall receive, your forbearance and indulgence for any *laches* on my part.

This Society by its very title is purely technical, professional, utilitarian; and I agree with your late President in regretting "that we have not yet enrolled in our ranks, to any great extent, that large body of private scientific workers who love and pursue the science of electricity without any thought of regarding it as a profession." He gave you the name of a Society that did this, and

in doing so took you back to dates earlier than those that I have been bringing before you. I shall have to take you to still earlier dates, to days when the utilitarian could raise the question—and it was more than once put to myself—What is the use of electricity? What do you expect to gain by it? Does it pay? There were no electric telegraphs then; no electrotype for instance; no plating and gilding by electricity.

The "London Electrical Society" was founded on May 16, 1837. It had a short life of six years, when it succumbed, as we shall see, to chronic atrophy. Its "lines" may not have been happily "laid," or it was premature, or the hands by which it was guided were not many enough or strong enough. If it may have been formed too soon, it certainly collapsed too soon—just at the time in fact when the power of electricity was making itself felt, in the practical realization of Cooke and Wheatstone's telegraphs. Had it held its ground only a little longer, it might have survived to this day, and have become a Chartered Society (as I trust ours will become) of which all of us might now be Fellows; electricity proper and electricity applied being cultivated by one and the same body corporate.

In the first Annual Address, delivered at a General Meeting held in the Theatre of the Gallery of Practical Science, Adelaide Street, Strand, on Saturday, October 7, 1837, I read: "In the spring of the present year I delivered a course of lectures on 'Electro-Magnetism and Magnetic Electricity,' at Mr. Clarke's, philosophical instrument maker, Lowther Arcade. At the close of each evening's lecture a conversation generally ensued among the gentlemen who honoured me with their attendance; and the want of a Society for the encouragement of electrical pursuits was occasionally spoken of and universally acknowledged. On the 16th of May a few of those gentlemen met, and, after some discussion as to the best mode of establishing such a Society, it was agreed that the attempt should be made. The first of our meetings took place on the 10th of June, and they were continued each succeeding Saturday until the 12th of August, the number of members gradually increasing the whole of the time. On each evening one or more papers were read, and several animated discussions took

place, visitors being allowed by the Rules of the Society to take a part."

These remarks were made by Mr. William Sturgeon, who had taken the chair at the May meeting referred to. He had been a non-commissioned officer in the Royal Artillery, and at the time of which we are speaking was "Lecturer on Experimental Philosophy at the Honourable East India Company's Military Seminary, Addiscombe, &c., &c." He does not tell us what he himself had been doing in the interests of Electricity, apart from his duties of office. He had established the *Annals of Electricity*, &c., the first number of which was published in October 1836. It was a half-crown octavo magazine. In the first instance it appeared quarterly, then the May number is followed by one in July, then a leap to October, and then monthly, and then otherwise. The publication ceased with No. 60, vol. x., in June 1843. It was followed by the *Annals of Philosophical Discovery and Monthly Reporter of the Progress of Practical Science*, of which only five numbers appeared, the first in July, the last in November, 1843.

In the *Royal Society's Catalogue of Scientific Papers* (1800—1863) Mr. Sturgeon appears as the author of sixty-nine papers—his first in Tilloch's *Philosophical Magazine*, in 1824, on "Electro-Magnetical Experiments;" his last in the *Manchester Philosophical Society's Memoirs*, in 1857, on "Some Peculiarities of the Thunder-storm which occurred near Prestwich, 16th July, 1856."

In Sturgeon's *Annals* alone will be found a record of the communications made to the London Electrical Society from the first ordinary meeting on June 10 to the meeting on October 7, 1837. They contain also full reports of the meetings; and the Papers read are printed in abstract or *in extenso*. Your President joined the Society on April 7, 1838; and on June 2 tried his 'prentice hand as a member on rather a knotty subject, "On the Effects of the Iron used in constructing Ships (especially that in Iron Steam Vessels) upon the Mariner's Compass," printed *in extenso* in the *Annals*, vol. iii. p. 1.

The General Steam Navigation Company had just then added to their fleet the iron steamship "Rainbow," which, I believe, is still

running; and my attention had been called to the very bad behaviour of the compasses in its voyage from Liverpool to London.

The founders of the Society, those present that is at the first meeting, were: Wm. Sturgeon, in the chair, J. P. Gassiot, J. E. Johnson, W. Leithead, and W. B. Lynn, five in all. "The Electrical Society of London" was the title first announced, which subsequently became "The London Electrical Society." Mr. Gassiot was appointed treasurer and Mr. Patrick secretary.

At the next meeting there were a couple of visitors, both of whom in due course became members, and one of whom, Mr. Pollock, was the author of the first paper read at the first ordinary meeting, June 10, 1837, "On the same cause under different circumstances producing the varied phenomena of the different Sciences of Electricity, Galvanism, and Magnetism." Of this paper no record remains; but the author wrote subsequent papers, in which he worked out his idea of a "Universal Principle." The first paper printed by the Electrical Society was also by him, entitled, "The action of the Voltaic Battery shown to be two-fold, and the distinction between the terms Quantity and Intensity determined by the Theory of Vibration; with a reply to the various objections made to the Theory."

This is a rather tempting and captivating subject to the members of this Society; but I fear it would hardly hold its own in these days of Volts, Micro-farads, Wheatstone's Bridge, Ohms, and Potential.

Before parting with Mr. Pollock, who was a well-known chemist of Fenchurch Street, I may mention as a curious fact, and as a singular mark of distinction, that he was never absent from a single Committee or Ordinary meeting from the time he first made his bow on May 20, 1837, until he signed his name as Chairman at the final meeting of the Committee for winding-up on October 28th, 1845.

The last ordinary meeting of the Society was held on March 21, 1843, and the last paper read was "On the action of Trees in condensing Atmospheric Vapour," by Charles V. Walker, hon. sec. The honorary secretaries in order were—Thos. Patrick,

Wm. Leithead, Jos. Jeffrey, and E. W. Brayley. The mean tenure of office with each was about six months. I followed Mr. Brayley, and remained in office until the Society was dissolved.

The ordinary meetings were held at first weekly, then fortnightly, and finally monthly. The days at first were Saturdays, subsequently Tuesdays. The place of meeting, free of cost, was at first the Adelaide Gallery, and finally, beginning on July 19, 1842, at No. 5, Cavendish Square, in a room rented of the Royal Polytechnic Institution at £25 a-year while there were less than twenty-five resident members—rent to increase as the strength of the Society increased.

The members of the Society were entitled as part of this arrangement to have free admission to the Polytechnic Institution. The Directors, on two occasions, on nights when the Institution was not open to the public, freely placed the large theatre and their apparatus at the disposal of the Society, where, on January 14 and February 18, 1843, lectures, well illustrated, were delivered before a large assembly of members and visitors "On Lightning Conductors" by your President, and "On Animal Electricity" by Dr. Letheby.

The Electrical Society were equally fortunate with ourselves. They were not only warmly welcomed and granted the free use of the theatre of the Adelaide Gallery—the place now in the occupation of Gatti, the confectioner—but all the apparatus of the Institution was at their disposal for experiment and illustration. I may mention, for the information of our younger members, that the Adelaide Gallery was a Polytechnic Institution in principle, where more scientific illustrations were to be seen by the general public than anywhere else in London. The advantage of meeting there was lost to us only on account of the Institution having been opened to the public on evenings, and the theatre being required.

There appear to have been no ordinary meetings between May 19, 1840, and April 20, 1841; and of course no papers read.

Soon after May, 1840, "One of the Committee" undertook to collect together all the MSS. that could be found of the Papers communicated to the Society since its foundation, and not yet printed in the "Transactions;" and to print them at his discretion

in abstract or *in extenso*, uniform with the papers already printed, and to lay before the Committee in one volume a record of the work done by the Society; which was completed and the volume ready on December 12, 1840. All arrears were now cleared off, and the history of the Society completed as far as existing materials would allow. A list of the missing MSS.—a very few only, as I have said—is recorded in "Sturgeon's Annals"; it is to be found in vol. i. pp. 502-3.

It was now arranged that the ordinary meetings should be resumed, and the Member of Committee who had edited the volume was invited, on March 24, 1841, to take charge of the affairs of the Society and to accept the office of Honorary Secretary and Treasurer; to arrange the meetings of the Society, and to publish, in such form as he should think best, and as promptly after each meeting as was possible or convenient, the "Proceedings" of the Society and the papers read. A guarantee fund was established, to meet the expenses in case of need. Meetings were resumed and held monthly, without any season of recess in the autumn; and the "Proceedings" were issued quarterly, each number containing the results of three meetings.

The best reference I can give you to the work done is in the royal 4to. volume of 210 pages, and the royal 8vo. volume of 565 pages, each with plates and woodcuts.

The subscription was one guinea for non-resident and two guineas for resident Members. The income from this, the chief source, was very small—£80 17*s*., £72 3*s*., £77 14*s*., or about £77 a year, is all that the books show at the best. The first list shows 76 names; but, as the result was only £80 17*s*., it is plain enough that all did not pay their due.

The result of my long experience, and no doubt it is more or less the same with many of you, is, that all Societies, whether for charity, church, club, or science, are afflicted with members more or less in number whose idiosyncracy is to pay badly or not to pay at all; and that a periodical process of weeding-out such unprofitable servants has to be carried into effect. It is to be hoped that the officers of this Society will not spare such men, nor be slow to deal with them. With some it is constitutional, with others it is—

one can hardly say what it is. But it is a growing complaint, and the more it is nipped in the bud before the case becomes serious the better. *Carpe diem* should be our motto in this. I mention this, having noticed that the amount paid into our Treasury falls short of the sum represented by the roll of Members.

I have said that 76 was the highest number of members on the roll, of whom some existed on paper only. I find elsewhere that 43 and 51 were the largest number of paying members. So that, in point of numbers as well as income, it was a very small Society; and yet we had good material among us; G. Lowe and James Whatman, Fellows of the Royal Society, joined us; and no less than nine, or one-fifth, of our small number subsequently had the honour of being elected fellows of the Royal Society. The names in order of election are: J. P. Gassiot, 1840; Dr. O'Shaughnessy (Brooke), 1843; R. Were Fox, 1848; Dr. Leeson, 1849; Captain Ibbetson and J. P. Joule, 1850; E. W. Brayley, 1854; C. V. Walker, 1855; and E. M. Noad, 1856.—[In passing I may add that F.R.S. appears seventeen times in the muster-roll of the Society of Telegraph Engineers]. Of the above nine, six remain; and three have passed away, namely, Captain Ibbetson, Dr. Leeson, and Mr. Brayley. A bye-law of the Royal Society was passed in 1847, limiting the number to be elected each year to fifteen only, which has since been confirmed, on St. Andrew's Day last. So that, of the above nine, seven had the still greater honour of being elected under the *régime* of the fifteen limit. Mr. Gassiot, who is first of the nine, was the Founder, to all intents and purposes, and the active supporter, of the young Society; and, outside the Society, he encouraged in purse and person the cultivation of Electrical Science. He was Treasurer for the first half of the life of the Society, and with few exceptions took the chair at their meetings. And here I must myself descend from the pedestal on which Mr. Latimer Clark inadvertently placed me in his address last year. The Electrical Society had no President, but elected a Chairman from among those present at each meeting.

Two members of the Electrical Society became Telegraph Engineers, the Rev. A. Bath Power and your humble servant; and Mr. J. H. Hammerton, who became Superintendent of Telegraphs

of the South Devon Line in 1846, on the staff of the Electric Telegraph Company, although not actually a member, was a frequent visitor at the meetings, and was also very much with Mr. Gassiot and myself.

Two sessions were completed and eight quarterly parts of the Proceedings published; but the prospects of the Society were not more promising under the new *régime* than under the old. The increase of members, and with them of income, was not appreciable; and on comparing notes it was found that the result of the two years' labour was a debt of 159*l.* 0*s.* 9*d.*, and no great assets. At the annual meeting, on April 8, 1843, the Secretary in his report, in reference to the want of success, said: "You placed me in almost despotic power over your interests; you gave me no council to guide and no committee to direct; you entrusted the management of your affairs to my unexercised judgment, and the expansion of the Society to my limited influence. Two years' experience has taught me that I am not sufficient to bear this double burthen;" and again, "for he (your Secretary) has for the last two years been virtually the Society . . . . members have very far fallen short of doing their part towards supporting your Secretary in his endeavours to advance your interests;" and "the term of my stewardship is expired, and I resign my functions as your Secretary and Treasurer; and again expressing my sincere regret that I have been unable to render in a better account of my stewardship."

The usual regrets and suggestions were made, but the Secretary said that all would be in vain "unless there were a standing Council of Members who would devote their time and energies to carry on the details of the Society." It was then resolved to call a special meeting to take into consideration the dissolution of the Society, which was held on April 22, 1843, when it was determined to dissolve the Society, and the requisite measures for carrying this into effect were ordered to be taken, and a Provisional Committee, to which Mr. Walker was requested to act as Honorary Secretary and Treasurer, was appointed. The result may be given in a few words, although it occupied two years and a-half to complete.

The members generally, and those on the guarantee list espe-

cially, were called upon, and in due course all claims against the Society were met; and on October 28, 1845, the final meeting of the Committee was held, two members, besides Mr. Pollock as Chairman and myself as Secretary, being present.

To complete the history, the Society being dissolved, the publication of the "Proceedings" ceased, but the materials and machinery for a quarterly journal remained, and every thing was in good working order. I was then strongly advised to take the matter into my own hands, and not allow the publication of electrical papers to drop. I did so, and established the "Electrical Magazine," following in order No. VIII. of the "Proceedings," by publishing No. I. of the "Electrical Magazine" at my own cost and risk, on July 1, 1843, and continued it to October 1, 1846, fourteen quarterly numbers in all. Volume I., of eight numbers, contained 628 pp.; Volume II., of six numbers, 480 pp. Here my courage and strength failed me. The burden upon time and purse was too heavy to bear single-handed.

What remain *now* of the nine years' labour are the four volumes which are in your Library, and the influence more or less which they, and the meetings reported in them, may have had in promoting and cultivating tastes for electric science. What remained *then* to the labourer was the pleasurable retrospect of the work in which he had been permitted to take so large a share, and of the congenial circle into which he found himself adopted, and a heavy balance of a printer's bill to pay.

To conclude. In what has been said I have studiously avoided taking you over ground well explored by and familiar enough to yourselves but untrodden by myself personally; and, at the risk of being tedious and of unduly trying the patience of some of you, have confined myself to fields of labour with which I have been personally very familiar—*quorum magna pars fui*—but the history of which is more or less inaccessible to you, and is very little known to the public of to-day. I would have made in passing a few more remarks than I have done upon some of the points that have cropped up in this very brief sketch; but I have passed them over, having detained you long enough already.

Mr. C. F. Varley: Gentlemen, we have heard a long and interesting address from Mr. Walker. During the first part of his paper, which dates back to 1845, I may say I am able to keep pace with him from my own recollection and my own experience, for it was about that time that I was introduced to telegraphy by Mr. Cooke, and assisted him in contracts with the Electric Telegraph Company in the following year. Mr. Walker's experience is large and special; and therefore the remarks he made, especially in the early part of his address, are particularly interesting to all telegraph engineers, because it was by the failures of those days that our present great success has been built up. Telegraphs began in 1845, and cost the large sum of 240*l.* a mile. The President has told you also, and I can bear him out in this, that Mr.—now Sir William—Fothergill Cooke spared no pains to get the best materials available at that time; and I do believe, myself, that at this present time, to take one instance, it is worth while—it is cheaper in the long run—to go to the expense of using the best charcoal wire than the common stuff now sold under the name of "Extra best best." It is an undoubted fact, and one which we should bear in mind, that a wire compact in its structure, like that of the best charcoal iron—homogeneous iron—does not disintegrate by rust so rapidly as a wire which consists merely of a bundle of loose fibres, and termed "Extra best best." Therefore it is well, and I am exceedingly pleased, to hear Mr. Walker give Sir William Fothergill Cooke the praise which is due to him for having used such excellent wire; and in illustration of this I may mention one fact in relation to the telegraph erected to connect the Victualling Yard at Gosport direct with the Admiralty in London. On the occasion of a storm in 1848, one of the poles on that line, 35 feet out of the ground, fell down, carrying the wires with it. The four wires were stretched no less than seven yards in the span of 100; and when we came to put up a new pole all we had to do was to wind up the extra quantity of seven yards of wire, and the telegraph line was as sound as before.

Mr. Walker has referred to a number of interesting facts. One which will especially interest this Society was that relative to the remarks made by my late and esteemed superior, Mr. Hatcher, as to

the impossibility of laying down wires in the streets of London. It is a singular fact that in 1846 I took a share in laying down wires to the premises 345 Strand, which was the first telegraph office opened in London, and those wires were successful for a considerable time. Mr. Walker also mentioned the fact that some of the original telegraph instruments—double needles—for which 45*l.* were charged to my knowledge, are still working to this day—another testimony to the excellence of the workmanship of Sir William Fothergill Cooke in the early days of telegraphy. The experiments that were made between Folkestone and the vessel moored out at sea may be looked upon as probably the first successful laying of a submarine line of considerable length. There is, however, an interesting fact which should not be lost to our recollection, and that is that in Russia a submarine line of nearly the same length was actually laid and mines were fired at a distance of two miles in the year 1818, and it is to that date that we must go back, I think, for the first idea of a submarine telegraph. I am very pleased that allusion has been made to the labours of Mr. W. Sturgeon, who was an intimate friend of my father's, and with whom I have had many interesting conversations and discussions upon telegraphy. In a large quarto work, a copy of which he presented to my father, which I think must have been a collection of his papers, there are numerous excellently executed copper-plates of various apparatus, which will bear inspection at the present day, and much may be gathered from them. It is unnecessary for me to say anything further than that I hope you will support the proposition which I am about to make, viz. that our worthy President will be kind enough to permit his address to be printed.

The motion having been unanimously carried, a suggestion was made that the original notices relating to the exhibition of the electric telegraph at Paddington read by the President should be photographed and accompany the address.

THE PRESIDENT: It is very gratifying to me to find that the address which I have read has been so heartily accepted. It must in many senses have been very dry, seeing it contains so many dates and so many matters of history, but I alone was in a position to collect and lay before you those matters of history, particularly with

regard to the London Electrical Society. As to the photographing of these curious handbills, that had been suggested to me by the Secretary, and when you see my address in print I have no doubt they will accompany it.

Mr. Latimer Clark: There is one duty which we must not omit to perform. The Society of Telegraph Engineers is as we know modelled upon the laws of the Institution of Civil Engineers. We have received the greatest kindness, courtesy, and assistance from them: they have lent us a helping-hand in many ways, but above all we are indebted to them for the continued use of their admirable room, which they are so liberal as to place at our disposal whenever we require it. I feel it is only necessary to propose a vote of thanks to the President and Council of that Institution to ensure its being cordially seconded and carried by acclamation.

Colonel Stotherd: I rise to second the proposition, and I am quite sure the kind permission to use this room has contributed very much to the prosperity of this Society.

The President: I can especially from this chair ask you to hold up your hands in favour of this vote, for this is not the first Society holding its meetings here of which I have had the honour of being president. For many years past the Meteorological Society has held its meetings under this roof, and they continue to do so still through the kindness of the Council of the Institution of Civil Engineers. Those who approve of the vote of thanks to the Institution will be good enough to hold up their hands, and I am sure it will be unanimous.

The motion was carried unanimously, and the meeting was then made special for the purpose of reconsidering Rule XII. It was resolved that the Ballot for the admission of candidates should take place in future at the first Ordinary General Meeting in each month in place of at every meeting as hitherto.

The Forty-second Ordinary General Meeting was held on Wednesday, the 26th January, 1876, Mr. C. V. WALKER, F.R.S. President, in the Chair.

The discussion of Mr. C. Fleetwood's paper, "On Underground Telegraphs—The London Street System," was resumed by

Mr. ALEXANDER J. S. ADAMS, who said: The paper read two meetings since possessed many points of interest, but few that were likely to elicit discussion. At the same time it appears to me that some such paper had become most desirable. We have no complete and connected account of the London street work of the past, but here is a basis upon which we may form progressive comparisons, and in this light the paper is of value. Much might, however, have been added to render the paper more complete than it is. For instance, the method for localising damp and minute faults; specimens of actual faults would have been particularly interesting, for I do not think a fault in covered work can occur without teaching its lesson. Some mention might have been made about earth-currents and other strange forces which would hardly fail to present themselves upon so extensive a system.

Looking at underground telegraphy in its entirety two important questions present themselves. The one, are the capabilities of the present system and the success achieved commensurate with the cost and the fact that this system has been developed upon the experiences of nearly thirty years? I may be corrected, but I believe that a new line in the heart of London would cost upwards of 2,000*l.* per mile, whilst I do not know any underground line, except comparatively new work, that has not sooner or later proved expensively troublesome.

Again, does the underground system of to-day bring us any nearer a solution of the problem of putting all lines under ground? To my mind the question of superseding suspended wires has long been one of considerable importance.

In these days of automatic signalling there are apparatus and force generators of the most complete and costly kinds—appliances probably equal to anything required of them for some time to come—but to what purpose are they if upon some meteorological change the insulation of suspended wires is reduced to almost nil, and the whole system rendered non-effective thereby? It is, of course, well to make the most of what we have, and to improve existing means; but suspended wires are practically beyond their time, and it would be wise to direct thought and energy towards the production of something electrically more constant. Gutta-percha or india-rubber are hardly available for the purpose. Gutta-percha may live to a considerable age under certain conditions, but how are those conditions to be provided and maintained?

In taking up the question of constructing a line electrically more constant than the present open lines I endeavoured to place the requirements of such a line as follows:—

It should be under ground.

Simple in construction.

Easy of repair.

Not liable to fail.

Free from joints.

Insulation constant.

Inductive capacity low.

Capable of carrying heavy power.

Upon trying various substances likely to form an insulator for the purposes of such a line I was satisfied with nothing so well as with asphalte, and to asphalte I directed my attention. It may be urged that asphalte has already been tried and has failed; but I think it was the constructive principle that failed and not the material. In one instance, I believe at Amsterdam, a trench was made, and at intervals along it sectional plates or diaphragms of asphalte were fixed and pierced with holes, through which wires were drawn and strained up. If with our beautiful cup insulators the wires suffer from surface conduction what shall be said of these exposed asphalte plates? In England again a layer of hot asphalte was put down, and in it lengths of old gutta-percha wire were imbedded, and upon this another

layer of hot asphalte. Now gutta-percha is the very worst material that could be used for such a purpose, inasmuch as gutta-percha surrounded by heated asphalte gives off a gas, which, inflating the insulating medium, produces air and vacuum holes; moreover the gutta-percha is reduced to so much sponge, and the under layer of asphalte would have partially cooled and become coated with moisture and grit before the top layer could be applied. Specimens showing this were laid upon the table at our last meeting.

In laying a provincial line with asphalte I should proceed thus, with (say) half an inch of insulating material between each wire:—

Open up a trench of sufficient depth and width, and in it lay a concrete foundation and sides two inches thick, and dress this concrete trough with hot pitch. The trough should be carefully made, and into it place a diaphragm of wood, perforated with equi-distant holes: pass the copper wires through the holes, and fasten them upon the one side. This board should be rigid. A second board, through the holes of which the wires have been threaded, is carried along the trough to a convenient distance, and the wires strained up by it. Hot liquid asphalte, of a specially prepared kind, should then be poured into the trough until level above the wires. A coating of hot pitch is applied, and when cold the top dressing of concrete raised conically to resist downward pressure. The whole is then filled in and carefully rammed with wooden punners. Some three years ago I laid down a short length of such a line, and before breaking it up some time afterwards cut out the section which was placed upon the table at our last meeting. I have also tested wires coated with an asphalte prepared by myself, under water pressure, with success, there being little or no absorption.

The system described by Mr. Fleetwood is evidently of recent construction, and therefore has to be proved; so that the present method adopted is still somewhat experimental. Suspended wires are, however, quite inadequate to the growing requirements of any healthy system of telegraphy, and my leading idea in rising is to direct attention to the subject.

I am sorry so many points of interest were omitted from the paper before us, for amongst other matters I should like to have learned from those able to judge if the underground system, as it is, is complete and a success.

The President: We are much obliged to Mr. Adams for his remarks as to the value of asphalte. It reminds me of days gone by, when Lord Dundonald—the Lord Cochrane of history—made some experiments in covering wires with asphalte. His lordship had a large property in Trinidad where bitumen abounded, and various experiments were made with a view of testing its adaptability to telegraph purposes. I myself in 1853 tried some wires covered with that asphalte; I did not test them in a scientific manner—roughly only: in due course they perished. I believe Mr. Latimer Clark has had some experience in the use of asphalte; and he will perhaps be good enough to state what the result was.

Mr. Latimer Clark: In listening to a paper on the London street work I felt I had been so long dissociated from such work that I was hoping to learn rather than desiring to speak. The paper was an able one and faithfully described the actual existing system. It treated on a subject of great importance to the Post-office and to some extent to submarine telegraph companies, but many interesting points appear to have been overlooked, and I agree generally with Mr. Adams as to what those questions are. I certainly felt surprise that no allusion was made to the use of paraffin and asphalte as insulating materials, and but very slight allusion to india-rubber. I was also in hopes that we should have had the results of some practical experience with lead-covered wires. These are points on which we require information, and on which I hope we shall yet have some from those who have had experience. There are some other points, viz. as to the best size of wire, with which the question of induction is connected, and it would be interesting to know what advantages result from using larger or smaller wire. We are aware, with reference to the rapid system of automatic signalling now coming into use, that the induction of street wires has an important influence on the speed, and we know that one of the benefits of asphalte is that it decreases the amount of induction, because we can use more of that material than of others which are

more expensive. In the early stages of underground work, when we became first aware of the great amount of retardation which occurs when a current is sent through gutta-percha covered wire, I took out a patent for encasing gutta-percha wires with asphalte to reduce the induction. I mention this to show that even in those early days the importance of speed of transmission was present to our minds.

With regard to asphalte, Lord Dundonald's idea was to cover copper with a kind of natural asphalte which came from his extensive bitumen lakes in the island of Trinidad. This material failed, partly by reason of its impurity and partly because it was so soft that the wires readily forced themselves through it to the surface. I then turned my attention to the very system which Mr. Adams has mentioned to-night. I laid down some experimental wires at Camden Town on level wooden troughs with diaphragms of porcelain or slate at stated distances, pierced with holes, and I carried the wires through the holes in little glass tubes, which were strung on the wires. Into the troughs I poured liquid asphalte. This made at first a promising piece of work, and, as far as the testing went, appeared quite perfect. I was, however, led to abandon that from circumstances which I do not remember now, and I tried the following plan, which also did not succeed. I had by me a large quantity of old gutta-percha wire which having been originally of impure quality had become entirely oxidised into the resinous state, and from which the gutta-percha had broken into little pieces like tobacco-pipes. I was reluctant to throw it away, so I laid up a number of these wires into a large cable and covered them with a casing of canvas. I then placed the cable in a large iron cylinder, five feet in diameter, and, exhausting the air from it, I got a good vacuum, and then let a large quantity of warm asphalte flow in and fill the cylinder. I next allowed the pressure of the air to come upon it. This, of course, saturated the cable with asphalte, and I found when it got cold a semi-solid mass, with the gutta-percha wires in the interior. The wires had not been affected by the heat, and the insulation was fair, but it made a nasty rope to handle; and, though it was covered with a second surface of sacking, it was never approved, and when I left the service was lost sight of. These two instances

constitute the only acquaintance I have with asphalte, but I have heard of its being extensively employed in France, and, as far as I know, with some success; so that I think the authorities in England would do well to inquire into the question and to make experiments for the purpose of ascertaining the real value of the material. I should have liked to know whether india-rubber could be used in conjunction with asphalte, because both asphalte and paraffin are materials which will not injure india-rubber. Then, again, vulcanised india-rubber would, I apprehend, form with these materials a very durable system of street work with very low inductive capacity; and I think further experiments might with advantage be made in this direction.

The French Atlantic Company have, I learn, laid down near Brest a double line of fifteen miles of lead-covered wire, manufactured in France. I am sorry Mr. Varley, who has, I believe, examined the line, is not here to tell us what the experience of it has been; but I believe that up to the latest accounts it had lasted extremely well and the insulation was very perfect. The chief objection to it was its expense; but as a means of preventing the gutta-percha from becoming oxidised lead is perfect, and I have seen specimens which appeared after twenty-five years as fresh to all appearance as new gutta-percha.

The effect of lightning on street wires is a point on which we might with advantage receive information. We all know that in time of storms our bells are rung by the inductive effects of the lightning; but how frequently faults in street wires are occasioned by lightning traversing the line is a question on which we should like to have the most recent experience. These are questions on which I hope this discussion will elicit some information.

The President: Mr. Clark has called attention to gutta-percha covered wires, enclosed in lead. If there is any gentleman present who has had experience in such work it would be instructive to hear about it. Many years ago—indeed, time passes so rapidly that I cannot say how many years it is*—the South Eastern Railway Company had a set of gutta-percha wires, covered with

* On reference to our books I find it was in July 1856. It cost 24*l*. per mile.—C. V. W.

lead, which were supplied to us by the Gutta-percha Company. They were laid under pavement, and when we last saw them, which was some years ago, they appeared to be as perfect as when they were first laid. When the office served by them was transferred elsewhere it was too expensive to pull up a long length of pavement at London Bridge Incline, and those wires lie buried there at the present time. If there is any gentleman present connected with the Gutta-percha Company we should be very glad to hear from him how this kind of wire has behaved elsewhere.

COLONEL GLOVER (responding to the President's invitation): Mr. Ffinch, who is present, has been longer connected with Indian telegraphs than myself; but on one occasion, being on a tour of inspection, I saw lying at Madras an immense quantity of lead-covered gutta-percha wire. There could not have been less than 50 to 100 tons packed on drums. I was informed that it was sent by Sir William O'Shaughnessey, and had remained where I saw it ever since its arrival. I had no opportunity of making a minute examination, but I had the wire on several of the drums cut open, and in almost every instance the gutta-percha was broken up into tobacco pipes, as Mr. Latimer Clark described. I can give no information of value with regard to these wires because I had no time to make any further examination; but perhaps Mr. Ffinch will be able to tell us more about it.

MR. FFINCH: I only know that the gutta-percha perished in the lead covering. Some of this description of wire was laid from Calcutta to Beypore, a distance of fourteen miles, and on one or two other short sections, and in every case it was defective through the gutta-percha having perished, but I believe the fault was in the inferior quality of the gutta-percha. There was, I know, a very large quantity of it sent out from England.

MR. LATIMER CLARK: Who manufactured it?

MR. FFINCH: I cannot say.

MR. LATIMER CLARK: Was it a leaden joint, with a short tube over it?

MR. FFINCH: It was one uniform joint. Each drum contained about a mile of wire.

MR. LATIMER CLARK: There was originally a large quantity of

Macnair's tubes used in telegraph wires, but your description does not agree with them.

Mr. SPAGNOLETTI: My experience in underground lines is not very great, as far as mileage goes. The longest length we have on the Great Western Railway is in the Paddington Station Yard, and there are here sixty wires in 4-inch iron tubes for a distance of a mile and a-half. The wires are in cast-iron pipes, and to get them clear of the roughness inside before putting them down a rymer was made, with two discs of steel about a foot apart at the end, and by passing it up and down it cleaned them well. The end of one pipe fitted into the socket of the next, and was filed round and smooth to prevent the wires from being damaged as they were drawn in. The pipes were then heated over a large fire, a rest being formed by a couple of rails, and old timber was used for fuel; when sufficiently hot they were dipped into a trough of coal-tar, which had the effect of japanning them inside and out and presenting a beautifully smooth tube. They were then laid down in a trench and cemented at the joints and the wires pulled through them. In the case of tunnels we carry the wires either over on poles or in sheeting attached to the wall.

The subject of the paper before us is an interesting one, and the telegraphic system of London is worthy of a paper to itself. The author has given us a good deal of information about these works and shows how London is served telegraphically, and the mileage is much larger than I anticipated. As the present underground wires seem to perish so soon, I fancy there must be some mistake on the side of supposed economy in using the wire we have done. Formerly some wires were put down of No. 16 copper wire, covered with gutta-percha to No. 4 or No. 3, and after having been down for a period of fifteen or sixteen years they were when taken up, consequent upon the alteration of the then route, found to be in a perfect state of preservation. The conductivity of No. 16 copper wire was always less than that of the wire used, and why a wire, No. 18, offering as it does still more resistance, is now generally used, I cannot understand.

The wire covered to No. 7 is much too thin, and is very liable in consequence to be damaged in being pulled through the pipes.

The author advocates the increase of underground wires; but in a paper by Mr. George Preece, which he read in 1873, was given a long catalogue of the disadvantages of street work, which would, I should think, deter any one from putting down more than could be avoided. The author does not mention the disadvantages of the underground system; but, in the case of the London street work, there is, I am told, one continual source of damage from the heat from bakers' ovens, by which the insulation of the wires is injured. As a parallel case, in the Paddington work we laid down the wires near to an unknown steam-pipe, by which they were considerably affected through being heated to an extent which almost destroyed insulation altogether; this was a serious source of annoyance, as we could not alter the course of the wires, unless a great outlay was incurred, and we had to devise a scheme to protect the wires from the heat of the pipe.

It has been a matter of surprise to me that the Government have lost sight of the Metropolitan Railway for their underground system, because that railway forms a perfect and secure route, and runs through the most populous parts of London. It would require only a short length of underground wire from the Post-office to Aldersgate Station; and, running eastward, they would get direct communication with the Great Eastern and North London Railways. Coming westward, at King's Cross they could run direct on to the Great Northern and the Midland Railways. At Gower Street, Euston could easily be reached to get the wires on to the London and North Western; and at Paddington they could join the Great Western and Hammersmith and City Railway, and then to the well-known Clapham Junction, from whence they could go anywhere. Going round the other way, from the City to the Mansion House, they could join the South Eastern, the Brighton, and the London, Chatham, and Dover Railways. I cannot but think that this route would be of great advantage, having testing stations so near and conveniently placed at the Metropolitan Railway Stations. There are manholes in the tunnels where a man could work safely if a system were adopted of putting the wires down in a trough along the side of the wall; and wires could be taken up, relaid, and tested very conveniently indeed.

With regard to a cable covered with asphalte, I made one of old, but good, gutta-percha covered wires, which were originally laid along the Blackwall line. The wires were wrapped in hemp, and a covering of canvas was put over the hemp and coated outside with Stockholm tar, and the whole was then passed through asphalte. That cable is working now. Some years ago—I think in 1863—Mr. Donald Nicol, who lived near Kilburn, had an idea of using Trinidad asphalte. He had it tried and laid down in blocks seven feet long, with a dozen wires in each block. The joints in the wires, which were very numerous, were made like a pocket corkscrew—one wire was spirally twisted, the other was straight, and the straight was pushed up the spiral one so as to give several points of contact. He sent a model of this to the Paris Exhibition.

I should like to ask Mr. Adams how he managed with regard to his joints, and what distance he anticipated he can thread wires through asphalte. I am afraid he will find some difficulty, and the number of joints must be very great indeed, and consequently not an advantage to any system.

Mr. Adams then proceeded to describe by illustrations on the board the plan of construction proposed by him. He said: The construction of a line I have already described; and supposing the trench with its concrete lining, and the wires are ready, the hot asphalte is poured into the trough over the wires from a boiler standing or wheeled along over the trench. The spout or boiler outlet would project into the trough, and this spout having a hot jacket, the asphalte is delivered on to the wires in quite a liquid state. Upon the hot asphalte running short, the end of the line is carefully covered whilst another batch is being prepared, when, after placing a properly constructed fire-box over the end to melt the last laid asphalte, the operation is continued as before. Thoroughly melting the end before commencing to pour the fresh asphalte renders the whole a continuation and not a joint. This plan has succeeded upon a small scale, and I consider it worthy of more extended experiment.

Mr. Higgins: How do you meet the difficulty of the expansion of the wires by the heat of the asphalte?

Mr. Adams: The board which you move along the trough should strain your wires up and keep them strained as the asphalte cools, and hard copper ought to be used. The specimen I put on the table showed that the asphalte clings to the wire very tightly. By straining up the wires with the board you have no difficulty from expansion.

The President suggested that in the event of a wire requiring to be removed there would be considerable difficulty in doing it, as he apprehended it would be necessary to melt the asphalte for the whole length of the wire which had to be withdrawn. He thought that introduced a great practical difficulty.

Mr. Latimer Clark said if experiments were made with asphalte it would be well to make preliminary trials as to the best description of asphalte for the purpose, inasmuch as there was great difference in the insulating power of different qualities of asphalte. In the ordinary road-asphalte there was a considerable quantity of ground carbonate of lime. It was very important to find out the best asphalte for the purpose.

Mr. Sivewright: I had not the pleasure of listening to Mr. Fleetwood's paper, and, as my experience of underground wires is somewhat limited, I am rather chary in expressing an opinion about them. Mr. Fleetwood recommends the extension of underground works, and I agree with him for more reasons than one. There can be no doubt that, so far as first cost is concerned, underground work is more expensive than open work; but, as regards the question of maintenance, I have never seen statistics brought forward sufficiently conclusive in favour of open wires, particularly for trunk lines. There is another point to be borne in mind, and that is, the great damage to which open wires are subject during storms. It was only last week that the Postal Telegraph system throughout the Midland counties was almost entirely paralysed from this cause. A violent storm swept from the Bristol Channel through the centre of England to the Wash, and wrought such damage that on Saturday morning there was scarcely a single wire workable to the north of England. Bearing in mind what enormous traffic passes over these lines, and how serious a loss the stoppage for a single day entails, we get a powerful argument in favour of laying down underground works even more extensively than has hitherto been done. And not only this; the improvements effected

in the insulation should not be lost sight of. In the wires connecting our large towns more leakage occurs within the first and last two or three miles of a line than in twenty or thirty miles beyond. Taking the system round about London, the insulation of the wires deteriorates more after coming to a point about five miles from London than it does over long distances in the open country. Smoke clings to the insulators, and fogs, especially in the neighbourhood of the Metropolis, bring down the average insulation of all the wires to a great extent. If underground work were substituted in place of open wires over these sections the average insulation would be improved. I do not argue that an underground can ever *supplant* an open system; but it may prove a powerful adjunct to the latter. Between large centres of business underground wires would probably in time pay their cost, if acting as nothing but as alternative routes: the improvement in working and cost of maintenance warrant open work being replaced by them in the neighbourhood of and through the large manufacturing towns. On these grounds I agree with Mr. Fleetwood that the extension of the underground system is desirable.

Mention has been made of gutta-percha wires inclosed in lead tubing. I have had no experience of this, but I have had some experience of lead tubing in underground work. Some two or three years ago I had occasion to make use of from 100 to 200 yards of covered wire of a new type, which was introduced and submitted for experiment. It consisted of copper wire covered with cotton. This cotton-covered wire was passed into a lead tube, and melted paraffin poured all round it by some means known only to the inventor. When tested it gave capital results, paraffin being such an excellent insulator, whilst the lead, chemically pure, was supposed to be indestructable. One hundred yards of this line was laid down on a circuit fitted with direct writers, and worked with an ordinary copper current. After being at work three months the circuit failed, and the fault was found to be in this section of experimental wire. On being removed the lead showed signs of having been attacked, and had in fact been eaten through in one or two instances by galvanic action, due to some impurity of the metal, or to some peculiarity of the soil. The tubing was returned to the inventor, and I never heard anything more about it. If gutta-

percha wire encased in lead tubing were employed to any great extent on a long circuit I should at first sight be inclined to dread the effect it would have in tending to lower the carrying power of the wire. The retarding influence already felt from induction would be greatly magnified, for now a veritable Leyden jar is introduced whose inner coating is the conductor, whose dielectric is the gutta-percha, and whose outer coating, the leaden tube, is in connection with the earth.

To Mr. Adams's plan I listened with great interest. It seems ingenious, but there are one or two practical difficulties which struck me. The first part is, how are you to maintain such a system as this? Supposing a fault occurs, and we must calculate upon that even if the asphalte is all but perfect, is the wire to be abandoned, or how are we to get at it? and again, if a new wire is required and has to be in the trough, how are you to do it? Will a new trench be required, or in what manner are we to add to the existing line new wires as they are required?

Coal-tar was mentioned in connection with gutta-percha, and Mr. Latimer Clark stated that under pressure he treated some old wire with coal-tar.

Mr. Latimer Clark: Coal-tar pitch.

Mr. Sivewright: All the experience I have had of any form of mineral tar, or any of the products of mineral tar, is that the gutta-percha deteriorates when brought into contact with it. Creosote is one of these products, and, although creosoted timber is now largely employed, yet it is invariably found that where gutta-percha wires are brought into contact with creosoted timber a deteriorating effect is produced upon the gutta-percha. Consequently all terminal poles and boxing for offices are plain timber.

The President: I believe Mr. Latimer Clark, previous to retiring from his official position with the Electric and International Telegraph Company, laid down lines of underground wires from Liverpool to Leeds. I should be glad to know whether those wires are still in existence.

Mr. Latimer Clark: I know they do not exist. I should myself like to know their history.

The President: Perhaps some gentleman can tell us something about them. I believe they were very good wires, and were well

laid. Mr. Latimer Clark has referred to induction in gutta-percha covered wires. I would ask whether any one present is aware of the result of the late Mr. Hearder's experiments at Plymouth. Mr. Hearder was entirely blind; but he carried out electrical experiments almost equally well with those who have the use of their eyes; and he was much mixed up with the experiments on the Atlantic cable as it lay at Devonport after the first unsuccessful attempt to lay it in the Atlantic. The inconvenience of induction was then made manifest on a very large scale for the first time. I have by me specimens of Hearder's wire, in which he hoped to reduce the induction by increasing the distance between the wire and the gutta-percha; to accomplish this the wire was covered with woollen or cotton thread of various thicknesses. I believe the results were not encouraging; but I am not aware to what extent experiments had been tried with it. If any one connected with the first Atlantic cable is present he might be able to tell us what the issue was. Colonel Stotherd, who has had great experience with torpedoes, for which buried wires are employed, may be able to give us some information on this subject.

Colonel STOTHERD, R.E.: I am afraid there is little that I can tell you on the subject under discussion, but I may refer to the use made of underground telegraph lines during the defence of Paris against the German Army in 1871. There are a number of detached forts round Paris, at distances of from 1½ to 3 miles from the enceinte, and forming an advanced line of defence. With these, telegraphic communication had been established previous to the investment. From head-quarters within the city, as a centre, lines of electric telegraph were carried to certain points on the enceinte, and to each of the detached forts. Each of the detached forts had also a line connecting it with its neighbours, thus forming a complete line round the city, outside the continuous line of works, but connected with the central station. Each telegraphic line was double, and consisted of an ordinary air-line carried on poles and an insulated cable buried beneath the ground. This double line of communication was intended to secure, as far as possible, certainty of signalling at all times. If the air-line was intercepted, the buried line might still remain efficient, and if both were destroyed

there was still an alternative, though circuitous, line through the communications to the right or left of each outlying fort.

The air-lines were very soon destroyed by shells, but the buried lines were seldom interrupted, and worked well during the siege. I do not know what precise form of electric cable was used, but one that I saw subsequently at Paris was similar to that made at Silvertown—Gray's core, covered with tape. There was no special care employed in laying this—in fact, everything was very rough; it was simply buried, without troughing, sufficiently deep below the surface to secure it from the explosions of shells.

As applied to torpedo work, I do not think there is much I can tell you which would throw light on the subject. Some curious results were however obtained in the course of a series of experiments on the firing of fuzes through half-mile lengths of electric cable laid side by side on the ground at Chatham in 1870. These were at first attributed to induction, but subsequent trials, under different conditions, gave probably more reliable information. This formed the subject of a paper forwarded to the Secretary, and which will no doubt appear in due course in our Journal.*

Mr. G. E. PREECE: The principal object of the paper which was read some meetings ago was to explain the present London system of telegraphs; and that becomes a question which is now more than ever impressed upon our minds, especially on account of the late snow-storms. It is now, within a few days, ten years ago when London was completely cut off from the rest of the world. I remember it very well indeed, and had good cause to do so. In January, 1866, the whole of our lines throughout the country were broken down. Communication to Edinburgh, Glasgow, and other places was carried on by the first line available; that was the southern line. We worked to the north by our wire to Gloucester and Birmingham. So, again, in the case of the breakdown of the last two or three days, we have been working from London to Edinburgh, Liverpool, &c. round by the west of England. We might as well work from Brest or Halifax. This points to the fact that, on the occurrence of snow-storms or heavy gales of wind, the telegraph communication of the country is liable to serious interruption. It should not be a question of

* *Vide Proceedings*, No. XII., page 410.

finance, or commercial business, but it is a question of the prosperity of the country. You require absolute certainty that the telegraphic communication shall be maintained unimpaired. I speak more as one who has had to do with main trunk lines which are secure from the eventualities of snow and wind. With regard to the Post-office telegraph service, we are gradually coming towards that point. Some years ago all our lines came overground by the railways to the London stations—to Euston, Waterloo, Shoreditch, &c. Now we are gradually extending the main trunk lines underground from the General Post-office to points outside the course of the London traffic. There was a diagram here on a former occasion which showed that. On the eastern side we now go to Stratford. Since that diagram was made we have carried out a new system of underground work as far as Stratford for the Eastern traffic; and in connection with the German submarine cables we have, I think, twelve wires across the North Sea. The same system has been extended to the South Eastern Railway in respect of the continental traffic; as regards the London and North Western Railway we have four or five miles to Kilburn; while to Hounslow we have the main postal route which was laid down about two years ago. In all directions the tendency is now to clear away from the streets of London all the over-house work and put our wires safely and quietly underground—thus giving better insulation, greater facility of working, and greater freedom from accident.

Mr. Latimer Clark made reference to the old electric telegraph wires which were laid in 1853. The Electric Telegraph Company laid a series of eight wires down the London and North Western line to Manchester and Liverpool. [Mr. LATIMER CLARK: And Leeds.] These wires were laid along the railway. They were No. 16 copper wire covered with gutta-percha to No. 4 gauge, taped and tarred.

Mr. LATIMER CLARK: What was the state of the wires when taken up—the better part?

Mr. PREECE: There was no better part, it was all equally bad.

Mr. LATIMER CLARK: I know the cause of failure when they got to Watford. It might be interesting to the meeting to hear it.

Mr. PREECE: I think there is a little bit left in the Rugby yard. It was entirely from exposure to the sun and bad workmanship.

Mr. LATIMER CLARK: Was the wire itself good when taken up?

Mr. Preece: We know that wires badly treated before they are put down must invariably be bad when taken up. Gutta-percha will not stand open-air work under any conditions. Subsequently to these wires being laid, in 1854, a very interesting work was carried out with gutta-percha wires in asphalte, under Mr. Latimer Clark, near Manchester. The wires were led through an insulator, and the troughing was filled with asphalte; the troughing was supported on posts in the open air, but through the action of the sun the wires failed. That was one of the earliest attempts I know of the application of asphalte for the insulation of wires. About the same time Mr. Newman, of the North Western Railway, applied asphalte and tar to the wires leading through the Lime Street tunnel at Liverpool, and those wires are still good.

The next question is with regard to old buried wire. I have seen some of the old cable that came from Dover which was laid down in 1850 in connection with the first cable, but my knowledge previous to that is not very great.

Mr. Spagnoletti referred to the enormous convenience which the Metropolitan Railway and its stations afford for carrying the underground wires of the Post-office right throughout London. We have, I believe, on the Metropolitan Railway some few miles of wire, but it has never been found desirable to increase the amount. The old Electric Telegraph Company, when I first remember it, went in for good solid wire. They used it at that time down Pentonville Hill, and it was laid down as far back as 1851-2. That was No. 4 gauge and 16 gauge copper. At present we are using the smaller wire called No. 7. The old wire when taken up was found to be perfectly good. The No. 7 is very small and very liable to injury, and its durability uncertain.

With regard to the extension of the underground system, inasmuch as we require great conducting power it is undesirable to use such very small wire as that, and I hope now we shall be able to use a larger size. There was another point mentioned, viz., the inconvenience of baker's ovens in the London street work. Now, there is no person who has to do with street work who does not look out for the bakers' shops in this survey, and he knows just where not to take his wires. In London we make a circuit under the *street itself where* we come to bakers' ovens. The

last point I will refer to is with reference to the use of asphalte, tar, bitumen, mineral products, &c. Now, I believe coal-tar is about the very worst thing that can possibly come in contact with gutta-percha. If you dip a bundle of gutta-percha wires into coal-tar the insulation will go down fifty per cent. very soon; if you leave it permanently in contact with coal-tar the insulation will go off entirely in the course of time. I have tried many experiments myself and have seen others carried out, and, though coal-tar is a preservative with regard to ironwork, yet with regard to gutta-percha it is quite the reverse. Stockholm tar is preservative, but it reduces the insulation, although it preserves the gutta-percha longer than would be the case without water; but coal-tar is about the very worst thing you can possibly apply.

Mr. ADAMS: I beg permission to reply to one or two points that have been raised. Any fault in one asphalte line would probably affect the whole. If you lay down an asphalte line there should be a sufficient number of wires to remove the necessity for additions, and I believe if we had even twenty such wires running through the country to-day the system would be practically worth twice its present value. I would use asphalte alone, and no pitch or tar. If pitch is used it should only be as a covering over the asphalte; the hot pitch would flow into the pores of the concrete. No doubt the best kind of asphalte should be used for this work, but my own experiments upon various asphaltes are as yet incomplete.

Mr. LATIMER CLARK: I hope I shall not be understood as recommending the use of coal-tar. I know it is fatal.

Mr. FLEETWOOD, in replying to the discussion, said: When the paper was read Mr. Varley referred to the practice of cutting and pricking wires, stating that when he was Engineer to the Electric and International Telegraph Company the men were forbidden to do it under pain of dismissal. Although this order may have been issued the bad habit has continued till a short time ago.

The small galvanometer used for finding wires was placed on the table when the subject was last before the Society. It is that generally used by the General Post-office for testing with Wheatstone's bridge. Mr. Varley, alluding to it, said he brought out a small instrument called "the wire-finder" in 1853, by which it was easy to distinguish any wire that had not been marked. I stated

that I had endeavoured to use it, but found it a very difficult task. Owing to the needle being hung on a fibre of silk, any wire passed into it through which a current was circulating made the needle move readily. On the table will be seen a needle, under a glass case. I have found by using a quantity current it is a very easy matter to find any particular wire, and it is suitable to put into the hands of our street men. During the last fortnight two wires had to be found in Fleet Street out of a bundle of one hundred, and then they were picked out in a very few minutes.

As regards india-rubber for underground work, I do not know of any at the present time in the London street work. As regards wires in Stockholm tar, a system of 24 wires was abandoned some time back at Camden: the wires were in a pipe filled with Stockholm tar, and in consequence of their failing a line of pipes had to be laid up Eversholt Street to the north end of Primrose Hill tunnel.

I mentioned in the paper that in my opinion the wires were frequently damaged by lightning. I have had several cases which have led me to form this opinion: the wires generally damaged are those on which the duplex circuits are worked, and I have thought it may be owing to the extra resistance at each end of the line; in most cases the fault has been at a joint where the wire has been badly centred.

In reference to the objections to underground works, I do not know of any myself that may not be overcome. The chief is that of induction, but there is no doubt that if we had a good line of underground wires from London to Manchester we should be able to solve the problem. It is to be regretted that the results of work done by inexperienced persons many years ago should be taken as a proof that underground wires are not suitable for long circuits and rapid working. The Underground Railway is not suitable for the system of wires passing through London. There are so many private renters whose places of business are in the busy thoroughfares that it is absolutely necessary our pipes should pass through the chief streets. I trust the time will soon arrive when wires overhead will be a thing of the past, at least in the Metropolis.

A vote of thanks to Mr. Fleetwood having been unanimously carried, on the motion of the President, the Meeting adjourned.

The Forty-third Ordinary General Meeting was held on Wednesday, the 9th February, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The PRESIDENT said: The author of the paper which is to be read this evening being abroad, the Secretary will read it, omitting certain pages of mathematical formulæ which it would be scarcely convenient to read, or which would be unintelligible to the minds of those who heard them. With the exception of these the Secretary will read Dr. Siemens's paper.

CONTRIBUTIONS TO THE THEORY OF SUBMERGING AND TESTING SUBMARINE TELEGRAPHS.

BY DR. WERNER SIEMENS.

The starting point of submarine telegraphy is to be found in the subterranean lines constructed in Prussia during the years 1847—1852. Before that time, it is true, attempts had been made to insulate wires with glass tubes, caoutchouc, &c., for use as underground lines. Experiments on a somewhat extensive scale by Jacoby* at St. Petersburg in the year 1842 deserve mention; but these attempts failed.

In the year 1846 the author suggested to the Prussian Government the use of gutta-percha as insulating material, this gum having then recently become known in Europe. Its remarkable plasticity and insulating property appeared to render it very suitable to the purpose in question, but neither the experiments made at Berlin nor those carried out simultaneously in England were attended with satisfactory results; for the joints of the gutta-percha

* *Pogg. Annalen*, vol. xxviii. p. 409.

(which was rolled round the wire) gave way after a short time. At last in the year 1847 the problem found its solution in a covering machine constructed and used by the author and Mr. Halske, by means of which the gutta-percha, rendered plastic by heat, is pressed round the wire without any seaming.

During the subsequent years an extensive network of underground lines, insulated with gutta-percha pressed round the conducting wires, was somewhat too hastily and inconsiderately introduced in North Germany and Russia; although those lines were far from durable (particularly because the conducting wires, to save expense, were imbedded in the ground without external protection, and at too small a depth) they afforded opportunity of gaining practical knowledge for the construction and maintenance of such insulated conductors and of studying their physical properties. But it was reserved for English enterprise to convert the knowledge and experience thus gained to practical account in a sphere where there was no competition with cheaper overground lines, that is to say, to submarine telegraphy.

As early as 1850 Mr. Brett laid a single gutta-percha-covered copper-wire across the Channel between Dover and Calais. As might be expected, this became useless: but in 1851 Mr. Brett substituted a conducting wire insulated with gutta-percha and protected externally by a sheathing of iron wires. This was the first practicable submarine cable.

No great difficulties were encountered in laying these cables in the comparatively shallow waters of the Channel. But when later Mr. Brett endeavoured to lay such cables in seas of greater depth, he failed, because the forces brought into play in laying deep-sea cables were not yet recognised, and consequently the necessary precautions had not been taken. The laying of the first successful deep-sea cable between Cagliari and Bona, in the year 1857, in which the author had been requested to take part, led him to investigate the mechanical principles of cable-laying.

As is the practice in England, the cable is stored in water-tanks on board the cable-ship, where it is coiled in a continuous spiral, and whence it is uncoiled, without kinking or being otherwise

impeded, over a pulley fixed above the middle of the tank. If we suppose the vessel to progress continuously with uniform speed in a straight line, letting the cable drop from her stern—the great length of the suspended cable admitting of our regarding it as perfectly flexible—every part of the cable will sink to the bottom of the sea with equal and constant velocity. Therefore, the distance of any part of the sinking cable from the surface of the water must be in proportion to the time that elapses from its leaving the ship. If the speed of the ship be constant, this time will be proportional to the horizontal distance of the ship, and the cable must form a straight line to the bottom of the sea. This straight line descends parallel to itself. Let every part of the suspended cable descend to the bottom by its weight in water, with the velocity v, the speed of the ship being c, then the angle a between the cable-line and the horizon is determined by the equation

$$(1) \qquad \tan a = \frac{v}{c},$$

If we assume that for a portion of the cable descending in the water parallel to itself the distance is proportional to the force w, the weight of the unit of cable in water may be resolved into two components, one of which, $w \cos a$, draws the cable towards the bottom in a direction at right angles to the cable-line, whilst the other, $w \sin a$, draws the cable in the direction of its axis and down the incline formed by the water, by which it is supported. The total action of the latter two forces is $w\, l \sin a$, l being the length of the suspended cable; or, as $l \sin a = h$, the depth of the water, the total strain, $P = wh$, is equal to the weight of the portion of cable hanging from the stationary ship vertically to the bottom.

If the cable is not retained in the ship by friction, the strain P is met only by the friction opposed by the water to the sliding of the cable in the direction of its axis. The amount of this friction depends upon the condition of the surface and upon the diameter of the cable. In heavy cables sheathed with iron, this amount, compared with the specific gravity of the cable, is so small, that the much greater portion of the strain P, or wh, has to be counter-

balanced by friction on board the ship, else the cable would descend too quickly and much of it be wasted.

In order to determine the necessary amount of this frictional resistance to be established on board the ship at any moment, it is necessary to know the depth of the sea at any spot where the cable is to pass, and to have a dynamometer indicating continuously the amount of strain with which the cable leaves the ship. Further, as the horizontal component of this strain checks the progress of the vessel, it is necessary to move the ship with a force sufficient to overcome this resistance and to give the required speed. Therefore, when the cable-ship had been provided with a brake of sufficient power and with a dynamometer which the author had constructed similar to a chain-balance, and when the cable-vessel (her engines being too small to overcome the strain of the heavy cable) had been taken in tow by another and stronger steamer, then the cable was successfully paid out between the previously mentioned places.*

Messrs. Longridge and Brooks† afterwards made extensive researches into the theory of cable-laying. The mathematical part of their treatise is not to be disputed, and gives an accurate description of the curve formed by a cable suspended in an oblique direction in water, if paid out with strain upon the sea-bed. But the physical part of the work, and the practical consequences drawn from it, are open to grave objections, as one of the first principles taken for granted, which materially influences the results, is incorrect. Further, the work is deficient as regards a clear perception of the principal factors and a lucid exposition of the results.

The forces acting on the falling cable are gravity and the opposing forces of friction. Of the latter there is first the sliding friction, which acts in the direction of the cable, preventing its sliding down the inclined cable line, and secondly the friction combined with displacement of water acting in a direction per-

* The meditations about the laying of cables, exposed above, represent the author's views on the subject at that time.

† Longridge and Brooks *On Submerging Telegraphic Cables.* (Proc. Inst. Civil Eng. vol. xvii. 1858.) This paper is reprinted further on in this part of the Journal.

pendicular to the cable-line. The latter force is proportional to the square of the velocity; the former proportional to the velocity itself. Longridge and Brooks supposed both forces to be proportional to the square of the velocity; they obtain therefore incorrect results, especially in determining the strength of the brake to be applied on board the ship. In stating the equation $\tan a = \frac{v}{c}$, the author also assumed the velocity of descent at right angles to the cable-line to be proportional to the force, but it will be shown later that this is almost exactly the case in those values of the angle a that generally occur in cable-laying. The author's supposition, that the cable forms a straight line if the speed of the vessel be uniform, does not depend upon the action of the frictional forces. The cable always forms a straight line of such inclination that of the two components of gravity the one acting in the direction of the cable is counterbalanced by the sliding friction and the brake on board the ship; the other, acting at right angles to the direction of the cable, is counterbalanced by the friction combined with displacement of water. This gives equilibrium of motion, and therefore the motion is uniform.

In the following:—

let a be the angle between the horizon and the direction of the cable;

,, ϕ be the angle between the direction of the cable and the direction in which every portion of the cable actually descends;

,, c be speed of vessel;

,, u be constant velocity with which the cable descends vertically through the water;

,, v be velocity of fall when the cable-line is horizontal;

,, w be weight of unit of cable in water;

,, h be depth of the sea;

,, l be length of the cable suspended in a straight line in water;

,, p be frictional-force or brake-force by which the cable is retained on board the ship;

let s be slack, ratio of surplus of cable paid out to the progress of the ship.

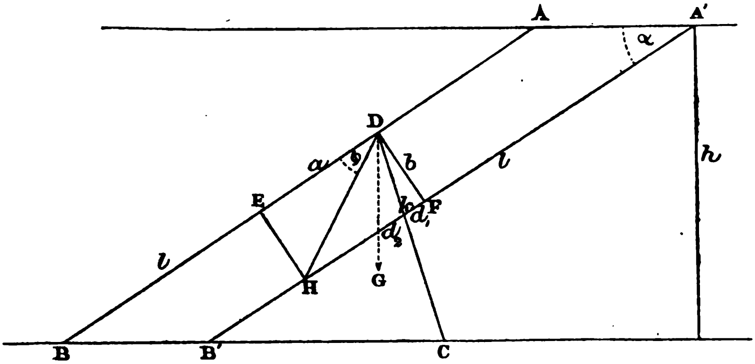


Fig. 1.

Let A′ B′ be the position of the cable; A B its position after the unit of time; and let the point D of the cable proceed to H in the unit of time; let the motion D H be resolved into the two motions D E and D F and the value of D E be equal a and D F $= b$. Then in both directions all forces must balance each other to have the actual velocity unvaried. Let the co-efficient of the sliding friction be, for the present, r; that of the other friction q. Then equilibrium of the forces acting in both directions D E and D F occurs, if

$$(a) \qquad l.\,w.\sin a - r.\,l.\,a - p = 0$$

$$(b) \qquad l.\,w.\cos a - q.\,l.\,b^2 \quad = 0.$$

Suppose the cable to be paid out without slack, the point D would arrive at C, the point H at K, making thereby B C = B D and angle G D K $= \frac{a}{2}$. Therefore is < G D F bisected by D K. If actually the point D has moved to H instead of K, then H K represents the surplus of cable paid out in the unit of time, and therefore $\frac{\mathrm{H\,K}}{c}$ is the value of s. Thus we have the following equations:—

$$h = l \sin a$$

$$b = c \sin a$$

$$s = \frac{\mathrm{HK}}{c} = \frac{d_2}{c}.$$

$$\tan \phi = \frac{\mathrm{DF}}{\mathrm{DE}} = \frac{b}{a}$$

The coefficients r and q must be replaced by the above-mentioned velocities u and v. If the cable *descends vertically* we have—

$$ur - w = 0, \; u = \frac{w}{r}.$$

If it *descends horizontally*, we have—

$$v^2 q - w = 0, \; v = \sqrt{\frac{w}{q}} \; ;$$

u, v, and w being constants of the cable, determined before the cable is laid. These are also the only three cable-constants that need be known for the present purpose.

Equation (b), combined with the other formulæ, gives—

$$\cos a = - \tfrac{1}{2} \frac{v^2}{c^2} + \sqrt{1 + \frac{v^4}{4\,c^4}} \; ;$$

therefore

$$(2) \qquad \frac{v}{c} = \frac{\sin a}{\sqrt{\cos a}} = \tan a . \sqrt{\cos a} \; .$$

This equation is strictly true. But *in practical cable-laying* the actual value of a generally is very small, and in this case equation (1) gives sufficiently exact results, as for small values of a we have $\sqrt{\cos a}$ nearly $= 1$.

We will first explain the consequences of (a) and (b) as they are determined by the exact value of $\frac{v}{c}$; and afterwards we shall be able to obtain simpler formulæ by inserting instead of $\frac{v}{c} = \tan a \sqrt{\cos a}$ for small values of a, the approximate value $\frac{v}{c} = \tan a$.

We have first the brake-force with any required amount of slack, determined by

$$(3) \qquad p = wh - \frac{c}{u} wh \; \left(\tan \frac{a}{2} + \frac{s}{\sin a} \right)$$

and if $s = 0$, the brake-power P without slack.

$$P = wh - \frac{c}{u} wh \tan \frac{a}{2}.$$

The first term wh of equation (3) is generally far more considerable than the rest, therefore *the brake-force is essentially equal to the weight of the cable* if the latter is supposed to hang vertically from the ship to the bottom. This value is also the maximum value of the brake-force, which is obtained approximately when the speed of the vessel is very great and the cable is paid out without slack.

From this maximum value two terms are to be subtracted, which we shall denote by P′ and S, where

$$P' = \frac{c}{u} wh \tan \frac{a}{2} \qquad S = \frac{c}{u} wh \frac{s}{\sin a}.$$

The signification of these terms is very simple. It is $P' = r\, l\, d.$ $S = r\, l\, d_2$, which is evident from (a) and (3).

d is the distance which the cable slides through in the direction A B if laid without slack; d_2 is the additional sliding caused by slack. P′ is therefore amount of sliding friction in the first case, S the additional amount in the second case. As $P = wh - P'$ the term P′ shows also how far in laying without slack the weight wh of the cable exceeds the amount of brake-force. It will be seen that P′ is almost *entirely independent of the speed* of the vessel, except it be very small, and that P′ is also *proportional to the depth h.*

S is *in proportion to the depth*, and, if the speed is not very small, *in proportion to the square of the speed.*

In order to demonstrate how far P′ and S depend on the other factors, especially on the speed, Dr. Froelich, to whom the author is indebted for the part he has kindly taken in these calculations, has given in the following table the values of P, P′, and S as they follow from the usual amounts of c. The depth taken is 2,000 fathoms, $s = 10\%$, these numbers referring to the Atlantic cable described by Longridge and Brooks, which has for constants

$$w = 0{\cdot}3208 \text{ lbs.}$$

$$\left.\begin{array}{l} u = 24{\cdot}201 \\ v = 3{\cdot}082 \end{array}\right\} \text{feet (English) per second.}$$

TABLE I.

c	2′	4′	6′	8′	10′	12′	15′
P =	3617·1	3607·1	3605·0	3604·7	3604·8	3604·8	3604·4
P′ =	232·5	242·5	244·6	244·8	244·8	244·8	245·2
S =	34·2	95·6	198·4	342·9	529·0	756·4	1173·3
wh =	3849·6						

The second table shows the considerable variations of brake-force with a definite slack, the latter being taken = 10%, and the depths those of the Atlantic.

TABLE II.

	c = 2′	4′	6′	8′	10′	12′	15′	wh
h = 500 faths.	p = 899·7	877·9	851·7	815·5	769·0	695·9	607·8	962·4
= 1·000 „	= 1799·3	1755·8	1703·3	1630·9	1537·9	1391·7	1215·5	1924·8
= 2·000 „	= 3598·6	3511·6	3406·6	3261·8	3075·8	2783·4	2431·0	3849·6
= 3·000 „	= 5397·9	5267·4	5109·9	4892·7	4613·7	4175·1	3646·5	5774·4

The angle ϕ, which defines the direction of the actual motion of cable, is expressed by the equation—

$$(4)\ \tan\phi \frac{\frac{c}{u}}{1-\frac{p}{wh}} = \frac{c}{u}\frac{wh}{p'},\ \text{when}\ p' = wh - p;$$

and finally s is defined by

$$(5)\ s = \frac{p'}{wh}\frac{u}{c}\sin a - 2\sin^2\frac{a}{2},$$

$$\text{or}\ s = \sin a \cot\phi - 2\sin^2\frac{a}{2}.$$

To facilitate comparison we now give the formulæ which, instead of (2)—(5), are the results of the exposition by *Longridge and Brooks*, describing them with our notation.

Formula (2) is the same, but we obtain instead

$$\text{of (3)}\ p = wh - wh\frac{c^2}{u^2}\frac{(1+s-\cos a)^2}{\sin a},$$

$$\text{„ (4)}\ \tan\phi = \frac{c}{u}\sqrt{\frac{\sin a}{1-\frac{p}{wh}}} = \frac{c}{u}\sqrt{\frac{wh}{p'}\sin a},$$

$$\text{of (5)}\quad s=\frac{u}{c}\sqrt{\frac{p'}{wh}\sin a-2\sin^2\frac{a}{2}}.$$

The difference between these formulæ and ours arises from the supposition that the force of sliding friction obeys the law of squares. This gives too high value for p, if, the speed of the vessel and the depth being known factors, the cable is to be laid with a definite amount of slack; and on the other hand this consideration will give too high value for s, if depth, speed, and brake-force be given factors.

We shall next see what modifications are obtained when we take formula (1) instead of (2), omitting the factor $\sqrt{\cos a}$, and when we in this way eliminate the angle a, which is practically almost indeterminable.

First let us compare the values of a defined by the above-mentioned two formulæ, several different values of c being given.

TABLE III.

$c=$	2′	4′	6′	8′	10′	12′	15′
(2) gives $a=$	68° 35′	41° 44′	28° 45′	21° 47′	17° 30′	14° 37′	11° 44′
(1) gives $a=$	57° 1′	37° 37′	27° 11′	21° 4′	17° 8′	14° 24′	11° 37′

The comparison shows that if the speed be more than 8′ per second, or about 5 nautical miles per hour, the numbers derived from (1) may be taken as sufficiently exact for practical purposes. Therefore in the following we shall take $\tan a=\frac{v}{c}$, omitting dimensions of the order $\left(\frac{v}{c}\right)^3$. Then we obtain—

$$(3')\quad p=wh\left(1-\tfrac{1}{2}\frac{v}{u}-\frac{c^2}{uv}s\right)$$

$$(4')\quad \mathrm{P}=wh\left(1-\tfrac{1}{2}\frac{v}{u}\right),\ \text{and}$$

$$(5')\quad s=\left(1-\frac{p}{wh}\right)\frac{uv}{c^2}-\tfrac{1}{2}\frac{v^2}{c^2}.$$

Equation (5) shows that *the slack* s is *inversely proportional to*

the square of the velocity, and that the first term of the value of s, which is the more important one, is *proportional to the difference* $wh - p$, that is to say, between the weight of the cable hanging vertically and the brake-force.

When the cable is being paid out, a variation in the value of s may occur from three causes: alteration in the depth h, of the brake-force p, or the speed c. If s be differentiated according to these three values and divided by s, we obtain the per-centage of the total variation of s due to the three variations in question;

$$\frac{ds}{s} = \frac{dh}{h} \cdot \frac{\frac{p}{wh} \cdot \frac{uv}{c^2}}{s},$$

$$\frac{ds}{s} = -\frac{dp}{p} \frac{\frac{p}{wh} \frac{uv}{c^2}}{s},$$

$$\frac{ds}{s} = \frac{-2\left(1 - \frac{p}{wh}\right)\frac{uv}{c^2} + \frac{v^2}{c^2}}{s}.$$

If, for instance, $h = 2{,}000$ fathoms, $p = 3{,}261{\cdot}8$ lbs., $c = 8$ feet, we have $s = 0{\cdot}10 = 10\%$; therefore

$$\frac{p}{wh}\frac{uv}{c^2} = 9{\cdot}9 \text{ and } -2\left(1 - \frac{p}{wh}\right)\frac{uv}{c^2} + \frac{v^2}{c^2} = -2{\cdot}1.$$

If now h, p, and c be each augmented by one-tenth of its value, s in the first case varies about $+ 99\%$ of its previous value, in the second about $- 99\%$, in third about $- 21\%$, giving instead of 10% slack

$$19{\cdot}9\%,\ 0{\cdot}1\%, \text{ and } 7{\cdot}9\%.$$

We see therefore that *the slack s varies in a much greater ratio than p, h, or c*, but *that the variations of s caused by those of h and p are much more considerable than those caused by variations of speed.*

Another important deduction is to be made from equation (4′), and this is, that, unless the speed be very small, *in laying without slack the brake-force depends only upon the depth*, and *is proportional* to it. This was shown also in Table I. *Vice versâ the depth may be determined from the brake-force*, and the annexed tables will show how far this can be accurately effected, com-

mencing with a speed of 4 feet per second. In Table IV. the brake-force P is determined exactly by the formula —

$$P = wh\left(1 - \frac{c}{u}\tan\frac{a}{2}\right).$$

Table V. shows the depths obtained from those values by the formula

$$h = \frac{P}{w}\frac{1}{1 - \frac{1}{2}\frac{v}{u}}$$

which is approximately correct. Suppose P is measured by experiment, and the cable-constants u, v, w, are known, the depth is found by the Tables:—

TABLE IV.

h	$c = 4'$	$6'$	$8'$	$10'$	$12'$	$15'$
500 fathoms	P = 901·8	901·3	901·2	901·2	901·2	901·1
1,000 "	= 1803·6	1802·5	1802·4	1802·4	1802·4	1802·2
2,000 "	= 3607·1	3605·0	3604·7	3604·8	3604·8	3604·4
3,000 "	= 5410·7	5407·5	5407·1	5407·2	5407·2	5406·6

TABLE V.

h (actual)	h (calculated)					
500 fathoms	500·3	500·1	500·0	500·0	500·0	500·0
1,000 "	1000·7	1000·1	1000·1	1000·1	1000·1	1000·0
2,000 "	2001·5	2000·3	2000·1	2000·1	2000·1	1999·9
3,000 "	3002·2	3000·4	3000·2	3000·2	3000·2	2999·9

It will be understood, even from approximate formulæ, that the laying of a cable with a definite amount of slack requires an exact knowledge of the cable-constants, as well as of the depth and speed. We may assume the constants of the cable to have been distinctly ascertained before laying it, but the measurement of depth and speed during the paying-out cannot be performed with an accuracy that will yield reliable results. The question now remains whether or not it is possible to overcome these difficulties.

It becomes necessary to ask, *Can a cable be paid out without brake-force?* We should in such a case have

$$s = \frac{1}{2}\,\frac{v\,(2u - v)}{c^2},$$

that is, *slack is dependent only upon the speed*, and not upon the depth.

It would be possible to pay out without brake-force and without slack if

$$2u - v = 0 \text{ and } v = 2u,$$

that is, if the cable had considerable sliding-friction, but in this case it would be impossible to lay with slack.

Suppose it were intended to lay with 10% slack and without brake-force, then in the case of the heavy Atlantic cable the speed must equal 26′·4; whilst the light cable of Longridge and Brooks (having the constants $v = 1{\cdot}404$, $u = 11{\cdot}024$, $w = 0{\cdot}06578$) would require a speed of 12′. Therefore as

$$c = \sqrt{\frac{v\,(2u - v)}{2s}}$$

we should be able by diminishing the specific gravity and increasing the sliding friction to construct a cable that might be laid without brake-force and with an amount of slack varying with the speed of the vessel.

There is still another means, proposed by the Author's brother, Dr. C. W. Siemens, of ascertaining experimentally what brake-power is required to obtain a desired amount of slack. This is effected by keeping the speed constant and increasing the pressure of the brake until further increase of pressure is no longer followed by decrease in the velocity with which the cable is paid overboard. Then we have found the brake-power necessary to lay without slack with a given speed, and it is easy to alter the pressure of the brake until any desired slack be obtained. But violent motion of the vessel, combined with irregularity in the velocity with which the cable runs out—an irregularity caused by the motion as well as by more considerable inequalities in the sea-bottom—will often render even this expedient ineffective.

To obtain precisely any desired slack, the only method will be

to pay out, with the cable, a rope or wire having approximately the same coefficients u and v as the cable. If this wire or rope be retained with sufficient brake-power to be paid out without slack, and, therefore, with tension on the sea-bottom, a counter will give the exact progress of the vessel, even without the errors caused by currents; therefore the brake has only to be maintained under the pressure that will give any desired ratio between the velocities with which the cable and the wire are run out. The greater expense incurred by the use of the wire will be more than compensated for, because the wire or rope paid out without slack does not measure the progress of the vessel in a horizontal direction, but the length of the ground passed over, and it therefore defines the amount of cable necessary to follow the inequalities of the sea-bottom without strain upon the cable; and, as the usual amount of 10% – 15% slack is generally paid-out, to avoid the danger of any such strain on uneven ground, and also the formation of catenaries of great length, it will be seen that the cost of the wire would be fully covered by economy in the amount of cable paid out.

In order to insure long service the insulation of a submarine cable or subterranean conductor must be perfect; that is to say, the resistance of the insulating coating must be equal to that defined by a calculation based upon the specific resistance of the insulating material employed. As soon as this resistance is observed to decrease it may be assumed that in one or more places the water has come into communication with the conductor through an opening in the insulator. This may happen during manufacture of the cable, but often it is first discovered during paying-out or some time after the cable is laid. Therefore, during the manufacture, as well as during the laying of the cable, and after it has been laid, its physical properties are submitted to continuous tests. If the existence of a fault is discovered, it is very important to determine its place or its distance from the ends of the cable. When the cable is being paid out it is necessary to arrive at this determination as shortly as possible, to enable the vessel (provided the fault be near the ship) to take in the portion of the cable recently paid-out, and containing the fault. The Author published in 1850* his

* *Pogg. Ann.* Bd. lxxix. p. 192.

theory of determining the distance of a fault. The principle of his method is, to obtain by means of two measurements of currents or resistance two equations, which will enable us to eliminate the unknown resistance of the fault, that is to say, the resistance which is opposed by the fault itself to the passage of the electricity to earth; it then being possible to define the ratio of distance of the fault from both ends of the conductor. The current may be measured at once from both ends of the insulated conductor, the distant end being once insulated, once in connection with the earth; or the determination may be made by two measurements from the same end, the distant end being in the first instance insulated, in the second measurement put to earth. Measurements of currents being less reliable and more difficult to obtain than measurements of resistance, the author subsequently* modified his formulæ based upon measuring currents, substituting those for measuring resistances as soon as he had succeeded in determining a definite unit of resistance which could be easily reproduced, and had employed this unit in the arrangement of an exact scale of resistances similar to a set of weights.†

Let $a\,b = l$ be the insulated conducting wire, of which the length and resistance are known; x and y the distances of the fault from a and b; and z the resistance of the fault. Then the distance x from the end a is determined by the following equations:

(1.) $\frac{x}{y} = \frac{w}{w_1}$ when both ends are in the testing-room, and w and w_1 are the bridge-resistances, no current passing through the galvanometer.

(2.) $\frac{x}{y} = \sqrt{\frac{a\,(c-b)}{b\,(c-a)}}$, a and b being the resistances measured from both ends, with the distant end to earth, c the resistance of the whole wire when faultless,

(3.) $x = \frac{a_1 - b_1 + c}{2}$, a_1 and b_1 being the resistances measured from both ends, with the distant end insulated.

* *Pogg. Ann.* Bd. xc. p. 1. Bd. xciii. p. 91. Bd. cxx. p. 512.

† Outline of the Principles and Practice involved in dealing with the Electrical Conditions of Submarine Electric Telegraphs. By Werner and C. W. Siemens. 1860.

(4.) $x = (c - b) + \sqrt{(b_1 - b)(c - b)}$, measuring from *one* end of the conductor for the determination of the distance of the fault.

In the first case the variable resistance of the fault, as well as the polarization there taking place, are entirely eliminated, because both determining measurements are taken simultaneously; this method, wherever it can be employed, thus determines the position of the fault with sufficient accuracy. But the case is very different when the ends of the conducting wire are at a great distance from each other, as in a submarine cable. The fissures through which the water comes into contact with the conducting wire are very small, often scarcely visible to the naked eye, and the resistance they offer to the passing current is extremely variable. Further, the polarization at these faulty places is often very considerable, and varies widely. Therefore measurements as indicated by the preceding formulæ seldom give satisfactory results, unless the fault be large, and its resistance consequently small.

Messrs. Clark and Jenkin have recently published two methods of determining the position of a fault in paid-out cables. These methods do away with much of the uncertainty to which the Author's previous methods were liable from the variation in physical properties of the faulty place. Mr. Clark insulates one end of the conductor and inserts between the other end and the earth a battery and a known resistance. Then perfectly uniform electrometers are employed to measure the difference between the potentials before and behind the resistance, and at the same time the potential of the insulated end of the conductor. The latter gives the potential existing at the fault. Now let w denote the inserted resistance; P and P′ the measured potential of its ends; p the potential at the fault measured at the other end of the conductor; and x the resistance of the conductor from the station where the battery is inserted to the fault. Then we have

$$P - P' : w = P' - p : x;$$

$$x = \frac{w\,(P' - p)}{P - P'}.$$

It being supposed that P, P′, and p are measured simultaneously, either in absolute measure or with perfectly uniform instruments, the variability of the resistance of the fault will not influence the

result. At the same time the influence of polarization at the fault is eliminated, which has only the effect of increasing the potential at the fault, and therefore acts like an increase of resistance of the fault.

The method published by Mr. Jenkin is based upon simultaneous measurements of the current passing through the fault and of the potentials of both ends of the conductor. To obtain this, a battery and galvanometer are inserted between one end of the conductor and the earth, the other end of the conductor being insulated. Besides this, both ends are connected with electrometers.

Mr. Jenkin's formula takes into account the decrease of current caused by the insulating coating, and, as in the case of slight faults, which are the most difficult to determine, imperfect insulation is of considerable influence, Mr. Jenkin's formula would be of great value, provided the simultaneous measurement of a current and two potentials, in absolute measure, at different places, and with sufficient accuracy to render the results reliable, did not prevent to a certain extent the practical use of it.

From what has been said, it follows that a method of determining faults cannot give reliable results unless it eliminates the very inconsistent resistance and the variable polarization of the fault. Further, in faults of great resistance, occurring in long conductors, it is necessary to take into consideration or to eliminate the insulation-current, that is to say, the absorption-current, or current of electrification, passing through the whole length of the insulator as far as the cable is faultless. Again, the method must be capable of being carried out easily and quickly.

The Author trusts these conditions are fulfilled by the following method:—

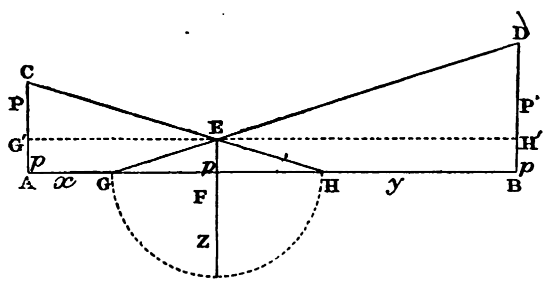


Fig. 2.

Let A B denote the faulty cable; F position of the fault, the resistance of which at the moment of measuring $= z =$ F G $=$ F H; A C $=$ P, measure of potential given to the cable-end by a battery inserted between A and earth; then C H will be the "fall" of the current passing through the fault and E F the potential at F, if the other end of the conductor at B be insulated. If we assume, provisionally, that the insulation of the whole cable, *except only of the fault*, is perfect, we shall have at B the potential p. If a straight line be drawn through G and E, then D B will be the measure of the potential P′, which shall give, when the cable is insulated at A, the same potential p to the fault F as it had previously by P from A. Now we have the triangles C G′E $\backsim$ D H′E, therefore,

$$\mathrm{P}-p : \mathrm{P}'-p = x : y,$$

x and y denoting the distances between the fault and both ends of the conductor A and B. As $x+y$ is the known length of the whole conductor, the position of the fault is perfectly determined. Suppose the resistance and polarization to be the same in both measurements made one shortly after the other, the results of the determination are not influenced by them. Nor does the imperfect insulation of the conductor alter the result, if the fault be in the middle of the cable or near to it. Otherwise, if the fault be nearer to one end of the conductor, it is easy to apply a correction which amends the result (with an accuracy sufficient for practical purposes). It is easy to obtain sufficiently accurate measures of the potentials if there is at each end station a sensitive mirror-galvanometer to which, by shunt-circuits, any desired degree of sensitiveness may be given, a very high resistance, of some millions of units, and the means of arranging a battery of defined electromotive force. If these batteries be constructed of Daniell's elements with sulphate of zinc, and care be taken that the zinc poles consist of homogeneous material and are well amalgamated, and that the liquids are of uniform strength, then the same number of such elements will have the same electromotive force provided their temperature be constant. If this is the case, and therefore any increase or decrease of the electromotive force by thermal currents arising from the contact of metals and liquids of different tem-

perature is avoided—*the electromotive force of such cells will be independent of their temperature.* Now, to both galvanometers may easily be given the same sensitiveness if each of them, together with the high resistance, and a battery of determined number of elements, is inserted in circuit, and the shunt-circuit of the galvanometer regulated so as to give the needle a deflection agreed upon by both stations. Inequalities of resistance of the batteries and galvanometers may be neglected, if, as is assumed, the resistances inserted are very high. If the galvanometers, brought to the same sensibility, are inserted between the ends of the cable, &c. and the earth, their deflection indicates the potential of the places of contact. A measureable alteration of the potential will not be produced by the shunt-circuit if the resistance of the batteries and of the whole cable is very small in comparison with it.

The measurements required by this method of determining faults are executed as follows: Station A inserts any battery between the cable and the earth. As soon as charge and polarization have become constant, A and B record their galvanometer-deflections, and then station A interrupts the contact of the cable-end with the free pole of the battery. Station B knows this by the decrease of deflection of its galvanometer. Now it communicates by means of conventional impulses of current the amount of deflection obtained to station A, and then the equal free pole of its battery is brought into contact with the cable-end. Station A now announces, by a signal agreed upon, whether its galvanometer shows greater or less deflection than that of station B. After this station B increases or decreases the electromotive force of its battery until A signals that the same deflection is observed. In order to control the results A and B may now alternately connect their batteries with the cable-end and vary the electromotive force of their batteries until each of them produces the same deflection at the distant end of the conductor. The electromotive forces of the batteries may be varied by increasing or decreasing the number of elements or by connection with shunt-circuits.

As may easily be seen, this method eliminates any inaccuracy arising from the conductive power of the insulator as long as the fault is in the middle of the conductor or near it. If the position

of the fault be very excentric, the elimination is only approximately complete.

Instead of eliminating, as in the preceding method, the errors arising from the physical conditions of the fault being liable to so great variation, by measuring from both ends as nearly simultaneously as possible—by which means the error or inaccuracy would be of the same amount for both measurements and one would compensate for the other—the result may be attained by reducing the potential of the fault to zero. If one end of an insulated cylindrical conductor be connected with the positive pole, the other with a negative pole, of galvanic batteries, the free poles of which are put to earth, the curve of tension cuts the cable in the middle, if the whole conductor is homogeneous and equally insulated, and the electro-motive forces of the batteries equal. By inserting and removing resistances between the batteries and the attached cable-ends, the position of that point of the conductor where there is *no* tension may be varied at will. When this point coincides with the fault, no current passes by the fault to earth, therefore the fault does not influence the intensity of the current at the cable-ends nor the form of the tension-curve.

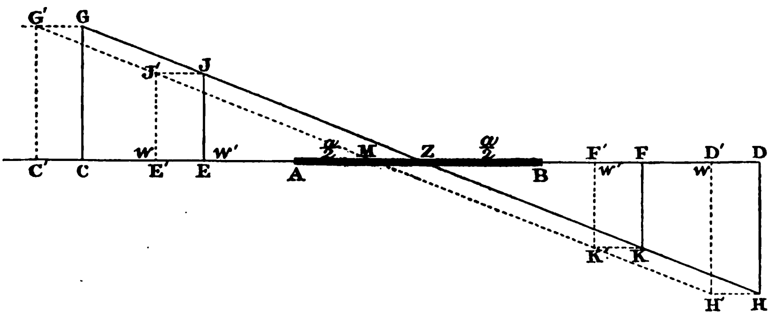


Fig. 3.

In figure 3, A B represents the cable, C E and D F equal resistances, E A and B F resistances equal to each other, but variable, G J Z K H the line of tension when the cable is faultless. Now, if a fault occurs at M, the potential difference G C — J E increases, and the potential difference D H — F K decreases. If, now,

station A increases its variable resistance B F until the previously measured potential difference G C — J E = D H — F K is restored at both stations, the dotted line G′ M K′ is the actual line of tension, and the resistance inserted in A and removed in B gives the distance of the two positions of the point free of tension, therefore also the measure of the distance of the fault from the middle of the cable. If the measurements were accurate, the resistance inserted in one station will be equal to that removed in the other.

The potential difference C G — E J, as well as that of D H — F K, may be measured as before by discharges of a condenser, the castings of which are connected with C and E and with D and F, or by deflection upon a sensitive galvanometer, the ends of the coils being connected by very high resistances with C and E and with D and F.

Hitherto we have dealt with faults of insulation, in which it was supposed that the conductor itself was not damaged and was continuous from one station to the other; but faults of another kind may occur. The conductor may be broken inside the insulating cover, or the cable may be perfectly severed, in which case, almost without exception, the ends of the conductor come in contact with the water. In the first case the distance from the fracture may be easily determined by measuring the capacity of the Leyden jar formed by one of the two parts of the cable, and by comparing it with the capacity of the unit of length of the conductor. This can be done either by directly observing the deflection of a mirror galvanometer, or, as suggested by De Sauty and Varley, by sending simultaneously the charge of the cable to be measured and the charge of the condenser serving as a standard of measure through the same circuit, and with the help of a Wheatstone's bridge or differential galvanometer obtaining an adjustment that shows no deflection. The ratio of the branches of the bridge will of course give the ratio of the charges.

These methods, which are very convenient in short cables, cease to be sufficiently accurate when the cables are very long. In the first place too long time is needed to complete the charge of the cable; secondly, the sensitiveness of the galvanometers must be reduced to too low a standard that they may measure the passage

of the large quantity of electricity accumulated in a long cable. The same objection occurs with De Sauty's method, because, if the galvanometer is too sensitive, the charge-current of the condenser, which is at first much the stronger, drives the needle over in the corresponding direction, whilst the charge-current of the cable, which flows more slowly, deflects it afterwards in the opposite direction.

The deficiencies of the methods at present known may be remedied in the following manner:—We determine the deflection from the discharge of a condenser of known capacity charged by a constant battery. Then we employ this condenser as a standard-jar in repeated partial discharges of the cable, and finally we measure the n^{th} discharge of this jar. Let k be the capacity of this standard-jar, the unit of cable-length being taken as unit of capacity; and let x be the capacity of the whole cable-length. Further, let P denote the potential to which the cable and the condenser are charged, P_2 P_2 P_3 . . . . . P_n the potentials of the cable, and of the connected condenser after the first, second, third . . . . . n^{th} discharge of the latter. Finally, let a and a_n be the discharge deflections of the condenser at the first and n^{th} discharge. Then we have

$$P : P_1 = x + k : x$$

$$P_1 : P_2 = x + k : x, \text{ and}$$

$$P_{n-1} : P_n = x + k : x;$$

therefore $$P : P_n = (x + k)^n : x^n,$$

or $$\sqrt[n]{P} : \sqrt[n]{P_n} = x + k : x$$

$$\frac{\sqrt[n]{P} - \sqrt[n]{P_n}}{\sqrt[n]{P_n}} = \frac{k}{x}$$

$$x = \frac{k \,.\, \sqrt[n]{P_n}}{\sqrt[n]{P} - \sqrt[n]{P_n}}$$

or, as a and a_n are the galvanometer-deflections corresponding to the charges of the standard-jar P and P_n, we have

$$x = k \quad . \frac{\sqrt[n]{a_n}}{\sqrt[n]{a} - \sqrt[n]{a_n}}.$$

It is much more difficult to determine the distance of the fracture of a cable if the end of the conducting wire comes into contact

with the water. Generally, when a cable breaks, the conducting wire and the insulating coating do not break in the same transverse section, so that either a portion of the wire lies uncovered in the water, or the wire comes into contact with the surrounding water through a narrow tube, only imperfectly filled with water. In the first case, measurements of resistance executed cautiously from the land will generally yield sufficiently accurate results. But, besides the variable polarization of the faulty place, there is in this case another cause of error, namely, that *the so-called earth-currents* almost always are to be found to a greater or less extent. Even when there is no *aurora borealis* visible at night, these currents (which must be traced to terrestrial or cosmical sources) often appear in cables in contact with water at both ends, and their intensity can amount to that of 6–8 Daniell's cells. The author succeeded in compensating for the influence of earth-currents upon the measurements by connecting the cable-branch of the bridge with a shunt of variable resistance, and with a sufficient electromotive force to balance the earth-current. The balance will be effected when the bridge-galvanometer indicates no current. Numerous observations upon earth-currents were made by this method, to which the author will refer at some other time. But, although we may in this manner avoid the influence of earth-currents in measuring resistances, the result is never reliable, as it gives only the total resistance of the cable and the fault together, and does not indicate the resistance of the latter. Often the resistance to transmission of the current from the conductor to the water is greater than the resistance of the entire cable.

In cables where the distant end is inaccessible, we cannot obtain a second equation, by means of which we might eliminate the variable resistance, and it is in this case better to measure the electro-static capacity of the portion of the cable.

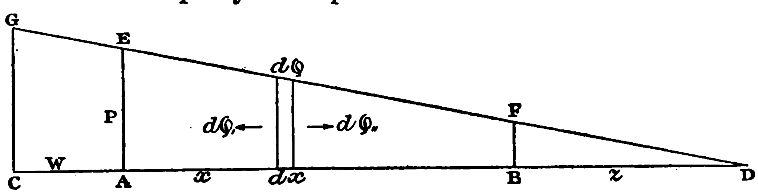


Fig. 4.

Let A B be a portion of the cable of the length l, the end B of which is in contact with water; $\mathrm{B\,D} = z$ the resistance of transition from conductor to water; $\mathrm{C\,A} = \mathrm{W}$ the resistance of the galvanometer by which the charge is measured; $\mathrm{A\,E} = \mathrm{P}$ the potential given to A, the end of the cable, by a battery inserted between A and earth. Then in the diagram, A B F E represents the charge of the cable.* Therefore at distance x from A the potential of the point considered will be $y =$ the ordinate of the point. If ϕ denote the quantity of electricity with which the cable has been charged, corresponding to the area A E F B, we have

$$y.\, dx = d\,\phi$$

and

$$\int_0^l y\, dx = \phi.$$

If, now, both ends of the conductor C D are connected to earth, and the electromotive force P at A (with which the cable has been charged) be removed, the quantity of electricity $y\, dx = d\,\phi$ will flow from both ends. Let $d\,\phi_{,}$ be the portion of $d\,\phi$ which returns to earth through A and C, and $d\,\phi_{,,}$ the portion going to earth through B and D. These quantities will be inversely proportional to the resistances they have to overcome. Therefore

$$d\,\phi_{,} : d\,\phi_{,,} = l + z - x : x + w.$$

and as

$$d\,\phi_{,} + d\,\phi_{,,} = d\,\phi$$

$$d\,\phi_{,} = \frac{y.\, dx\,(l + z - x)}{w + l + z}.$$

Further $$y : \mathrm{P} = l - x + z : l + z,$$

therefore $$y = \mathrm{P}\,\frac{l + z - x}{l + z}$$

and we have $$d\,\phi_{,} = \mathrm{P}\,\frac{(l + z - x)^2}{(w + l + z)\,(l + z)}\,dx.$$

or $$\phi_{,} = \frac{\mathrm{P}}{(w + l + z)\,(l + z)} \int_0^l (l + z - x)^2\, dx.$$

$$= \frac{\mathrm{P}}{3\,(w + l + z)\; l + z} \Big((l + z)^3 - z^3 \Big)\,..$$

* *Pogg. Ann.* Bd. lxxix. p. 499, 1850.

If the value of $a = l + x$ (determined by measuring simultaneously the charge of the cable and the resistance) be substituted, we have

$$\phi_{\prime} = \mathrm{P}\frac{a^3 - z^3}{3(w + a)\,a}$$

hence $$z = \sqrt[3]{a^3 - \frac{3\,\phi_{\prime}\,(w + a)\,a}{\mathrm{P}}}$$

As we have $\mathrm{P}\,l = 2\,\phi$, the quantity of charge of the entire cable when faultless, as we have also $\mathrm{P} =$ the charge q, of the unit of insulated cable,

$$z = \sqrt[3]{a^3 - \frac{\phi_{\prime}}{q} 3\,(w + a)\,a}$$

As by measuring the resistance simultaneously we have ascertained a, and as $l = a - z$, the length l of the broken cable is determined. If $z = o$ and $a = l$, the preceding equation gives

$$lq = 3\,\phi_{\prime}\,;\ \phi_{\prime} = \frac{lq}{3} = \frac{2\,\phi}{3},$$

that is to say, if a charged cable which is at one end connected with earth, without resistance, be at the other end also connected to earth, and without resistance, then *two-thirds of the charge returns to the charging station, and one-third goes to earth at the distant end.*

Of course no time must be lost between the removal of the battery and the insertion of the galvanometer (put to earth), because, during the time the cable remained insulated, a considerable portion of the electricity would travel to the other end; therefore, the discharge would be too small. But if the change of connection is made at one and the same moment (as effected by Helmholtz in 1851), the results obtained by this method correspond very closely, and are very exact. If the conductors to be measured are of great length, the retardation of the current in consequence of the charge introduces error. Therefore, if this be the case, the preceding formula needs correction, owing to this retardation of the current, but the author has not as yet succeeded in determining the necessary correction.

The PRESIDENT: This paper is one which can only be properly appreciated by reading it quietly, with time to study the diagrams and formulæ, most of which have necessarily been passed over. For myself, I have no personal familiarity with submarine cables, and therefore should not be justified in occupying your time by any remarks of my own. The paper commenced by referring to the first cable which was laid across the British Channel. It was manufactured for Mr. Read the elder. I remember seeing the cable at Dover. It was at that time scarcely believed that a cable was about to be laid across the channel; and I have in my possession at this moment—and it forms an interesting matter of history—a letter from Mr. Lewis Ricardo, the then chairman of the Electric Telegraph Company, asking me if it really was true that a cable had been made, and was about to be laid across the channel? There are several gentlemen present who are acquainted with the two chief points to which your attention has been directed, viz. the mechanical operation of laying cables and the electrical operation of testing for faults; and I am sure the remarks of those conversant with these points will be listened to with great attention and great patience by the Members present.

Mr. WILLOUGHBY SMITH (responding to the President's invitation) said: As electrician to the Telegraph Construction and Maintenance Company, I have had a large experience in submarine telegraphy, but I am at the same time happy to say my experience in localising faults has been very limited. The paper commences by stating: "The starting point of submarine telegraphy is to be found in the subterranean lines constructed in Prussia during the years 1847 and 1852." Now, I question whether, prior to 1850, any reliable data were obtained from the then very imperfect state of subterranean lines in England and other countries. The successful laying of the line from Dover to Calais, in September 1850, was the germ from which submarine telegraphy sprang.

Then, again, Dr. Siemens says: "In the year 1846 the Author suggested to the Prussian Government the use of gutta-percha as insulating material: this gum having then recently become known in Europe." Further on it is stated, that in 1847 the Author in

conjunction with another person constructed a machine by means of which the gutta-percha, rendered plastic by heat, was pressed round the wire without any seaming.

I have with me a few notes, which I should like to call attention to; and, if I am wrong in anything, I hope there are gentlemen present to correct me. I do not think anyone will dispute that it was in the year 1843 that Dr. Montgomerie transmitted the first sample of gutta-percha to the Society of Arts. Samples of several articles roughly made from gutta-percha were exhibited at the Society of Arts in 1844. The first patent in which gutta-percha is mentioned was obtained by Mr. Charles Hancock in May 1844 "For certain improvements in corks and other stoppers." In 1845 and 1846 patents were obtained for mixing gutta-percha with all known and unknown substances for all known and unknown purposes. The most important patent of the year 1845 was that of Henry Bewley for manufacturing gutta-percha bottles, tubes, &c. In his specification he says: "The gutta-percha in a plastic state is put into a cylinder, and a piston working into this cylinder presses the gutta-percha against a heated disc in which there are holes through which the gutta-percha is pressed into a cup, from whence it passes out and round a core, and descends in the desired tubular form into a receiver of cold water." This, no doubt, was the germ of the present tube and wire-covering machines. In February 1848 Professor Faraday called public attention to the electrical qualities of gutta-percha. In the paper under discussion Dr. Siemens says that in 1846 he suggested to the Prussian Government the use of gutta-percha as an insulating material. It is curious that Professor Faraday—considering how careful a man he was—should, two years after, bring it before the public as a new thing. I would refer you to a letter on this subject published in the *Philosophical Magazine* for February 1848. There we find that the first patent in which gutta-percha is mentioned in connection with electricity is by Charles Hancock, in May 1848, in which he says: "Gutta-percha previously boiled with muriate of lime is passed between heated cylinders while rosin is sifted on, or solutions of the two are made and mixed. This is employed where complete electric insulation is desirable." In September 1848 John Lewis Ricardo patented

"Improvements in Electric Telegraphs and apparatus connected therewith," one of which was "Laying one or more wires between bands of gutta-percha, and then through a pair of grooved rollers heated by steam;" but he preferred to use a compound composed of gutta-percha, New Zealand gum, and sulphur. The Gutta-percha Company had been, since early in 1848, endeavouring to obtain a perfect method of covering wires with gutta-percha for telegraphic purposes. In 1849 (I think in November, but am not quite sure) they commenced to supply the Prussian Government with "sulphuretted gutta-percha" covered wires. No wonder the insulation was soon destroyed, considering the large quantity of sulphur they had mixed with the gutta-percha. In 1849 the Gutta-percha Company commenced to manufacture the line which was laid from Dover to Calais in 1850; and in 1850 Ernest Werner Siemens patented a machine for covering wire with gutta-percha for telegraph purposes, which machine was much inferior to those already at work on the Gutta-percha Company's premises. It was in September 1850 that Mr. Read laid the wire from Dover to Calais, and it soon failed, as might have been expected. I believe it is not generally known that the shore end of that wire was composed of a copper wire covered with cotton, passed through a solution of india rubber and enclosed in a thick leaden tube. The shore ends were laid some days before the gutta-percha covered wire was laid. I believe but for those shore ends the wire would have lasted longer. Up to the time of joining with the wire in the lead tube the cable lasted well, but when we arrived at the lighthouse at Cape Grisnez we could not get any signals, and in trying next day to remedy the evil the wire broke.

The first channel cable was laid in 1851. In 1854 the Mediterranean cable was laid between Corsica and Sardinia, in some places at a depth of 500 fathoms. Had Mr. Brett employed a steamer instead of a sailing vessel to lay the other section from Sardinia to Africa in 1855 I think he would have been as fortunate as he was with the line between Sardinia and Corsica, but by using a sailing vessel he had no control and consequently lost the cable. It was a very heavy cable to attempt to lay at depths of 1,500 fathoms, but in 1857 Mr. Newall succeeded in laying a light cable over the same ground.

Mr. C. F. Varley, F.R.S.—This paper only came into my hands this morning, and though I endeavoured to get through it, and spent about three hours and a half over it, I have not succeeded yet in dealing with the whole of the equations in the paper. Nevertheless there are many points I can deal with, and I am sorry that it will be my duty to attack the paper from the two very first lines. In these it is stated, "The starting-point of submarine telegraphy is to be found in the subterranean lines constructed in Prussia during the years 1849—1852."

Now I may be permitted to say that the starting-point of submarine lines was to be found when this century was in its teens, at St. Petersburg, by, I think it was, Professor Sömmering (if you consult Dr. Hamel's little book you will see the particulars described, as well as the authority whence he derived his statement), that somewhere about the year 1807 or 1808 that Professor introduced to the Russian Emperor a little subterranean line, with a charge of powder at a distance of more than a mile. The Emperor was told to take two wires in his hand, and was informed that on making them touch the mine would explode: he did so, and the mine exploded. That led to a great number of experiments, and we find when the allied generals were assembled in Paris in 1815 the same Professor amused the English and Prussian officers and others by laying his line across the Seine and firing charges of powder on the other side. In 1818, I think I am correct as to the date, Professor Sömmering had a telegraph at work, and so confident was he of success with it under the sea that a submarine cable was ordered to connect Cronstadt with St. Petersburg. This I think was the first and the parent idea of submarine telegraph cables for sending messages.

When we come to the practical realisation of submarine telegraphy I think the Electric Telegraph Company will be able to claim priority over every one else. In the year 1847 Mr. Charles West brought to us wires insulated with india-rubber, he having previously established communication between Dawlish and Teignmouth through the tunnel, by means of such wire partly buried in the ground and partly on land or the sides of the tunnel. I was at that time in the south of Devon, and, the wires being

exposed to the sun and open air, the insulation was very bad. Mr. West, in October or November, 1847, thought he should like to try the possibility of working under the sea by means of an india-rubber covered wire, and my superior officer, Mr. Reece, Mr. West, and myself arranged to test it by means of some of the wire which was afterwards erected on the line. A boat was got ready: Mr. West's man was left on shore with a single needle instrument, and Mr. West, Mr. Reece, and I went in the boat. We paid out this wire by hand: telegraphed through it and tested its insulation. We then returned to the shore, I being very sea-sick, for I must confess it was my first trip on the "briny." That was I think the first time a signal was sent under sea-water.

Next, in the year 1848, the Admiralty desired that their telegraph circuit, which extended from Whitehall to the Victualling Yard at Gosport, should be extended on to Portsmouth. Now, I may tell you that as early as the year 1846 they had expressed a similar wish to Messrs. Cooke and Wheatstone, because the semaphore telegraph between Portsmouth and the Victualling Yard was often obscured by fog, while the electric telegraph between the Victualling Yard and Whitehall remained in perfect working order. I remember, on taking charge of the London and South Western Railway telegraph, finding to my surprise at Gosport about a mile of wire in a leaden tube. The wire was covered thickly with cotton and insulated with a mixture of pitch and resin, it being intended to lay this wire from the Victualling Yard to Portsmouth Dockyard; this, however, was never carried out, although there was an attempt to lay a submarine wire of more than a mile in length. In 1848 wires were laid across the upper portion of the harbour, and those wires were insulated in a different manner as an experiment. Mr. Willoughby Smith has told you that Mr. Ricardo patented a process of insulating wires between two bands of gutta-percha. Half-a-dozen wires were laid uniformly distant upon a sheet of gutta-percha, another sheet of hot gutta-percha was then placed over them, and the whole pressed together. Two such ribands were laid down, as also a leaden tube with eight wires insulated with india-rubber. Most of the gutta-percha wires failed from the two sheets of gutta-percha

not being properly united; but still some of the wires were in working order till the following year, when a thunderstorm came across the line and the land lines were struck at a distance of ten or twelve miles from Cosham, and the wires were burst open. The late Mr. Read had at that time a contract with the Company to keep these wires in order, and I was at that time under his orders. Accordingly, two lengths of gutta-percha rod, about five-eighths diameter, each rod containing three conductors and two sets of pipes, were substituted for these other wires, which were taken out. This was an actual submarine cable, working in a circuit from Portsmouth to Whitehall, London, and was laid in 1848 or the beginning of 1849. It was in 1848 I took charge of this telegraph, and it was laid shortly afterwards.

Thus, then, we find the idea of telegraphing under sea-water began at an early date in this century, and was perfectly realised by the Electric Telegraph Company as early as the latter part of 1848 or the beginning of 1849. The most that can be claimed for the wires which were laid down in Prussia—the gutta-percha covered wires—was the introduction of gutta-percha for underground not submarine circuits; and Mr. Cooke we all know, many years before, had laid down subterranean wires sufficiently insulated for working. I ought to mention when we took up these leaden tubes containing the three india-rubber wires from Cosham we found that some malicious individual had slit open the leaden tube and partially damaged the india-rubber; but, inasmuch, as he had not quite cut through the india-rubber, insulation was sufficiently perfect to work till the wires were destroyed by lightning. Seeing that we have greatly perfected the manufacture of india-rubber covered wires we must not give gutta-percha the entire credit, although it must have the largest share of it in the construction of all deep-sea cables.

I have noticed another remark in the paper, in which the author does not entirely but almost entirely claim the introduction of tanks of water on board ships for carrying the cable. That is essentially an English invention or introduction, and I take this opportunity of claiming for my country that most important improvement in the submerging of telegraph cables, viz., that of

sending them out under water and keeping them in water from the moment they are manufactured till they are laid in their final resting-place. The faults of some of the West Indian cables of comparatively recent date was due almost entirely to the neglect to attend to this precaution, a precaution which was insisted upon by the Privy Council with regard to the construction of deep-sea telegraphs in the report which was issued in 1860, after the Committee had sat for a period of two years.

I would like, before I go to another subject, to suggest to authors of papers of this description—although it may be rather a bold thing to do—still I would suggest that they should put into their formulæ some of the intermediate steps by which they arrive at their final equations. It would save an immense deal of trouble in interpreting their papers.

The first part of this paper—I mean that portion which refers to the mechanical operation of laying cables—is chiefly confined to a description of a clearly-written paper by Messrs. Longridge and Brooks, read before the Institution of Civil Engineers. Now, when we remember the date at which that paper was written,—I think it was about 1856 when submarine telegraphs were as yet in their childhood,—and when we bear in mind that that paper was published then, and that, notwithstanding the time which has elapsed and the experience which has been gained, there is only one point with which Dr. Werner Siemens has to find fault, I for my part think that it is a very high compliment to the value of that communication. When, therefore, Dr. Siemens makes the assertion that the friction of a cable through the water varies as the rate, and not as the square of the rate, I think it is a pity he has not given us some data collected from the experiments which he has doubtless made, instead of making the bald assertion that the friction varied as the velocity and not as the square of the velocity.

And whilst on this point I will draw attention to a circumstance to which I had my attention first drawn in connection with the Atlantic cable, and that is: granting for the sake of argument that Dr. Siemens is correct when he states that a smooth cable passing through the water experiences a friction in proportion to

its speed, still these are not the conditions of a cable like the Atlantic passing into the water. That cable, as you know, is covered with a certain number of steel wires covered with hemp, and coated with a substance known as Clarke's Mixture. Now, the whole of this presents a smooth exterior, but the moment the cable is passed round the drum it becomes flat in shape, and you have a number of fibres sticking out of the cable.

We find that the cable, instead of being smooth, is now bristled all over with filaments. Now, a cable going down horizontally through the water experiences retardation in proportion to the square of the velocity. Each of these fibres is in a position perpendicular to the cable, so that while the greater part of the cable will obey one law—the results perhaps varying with the speed—each of these hundred thousand filaments obey the other law; and it is a pity we have not experimental data to prove how far one law predominates over the other.

During the paying-out of the 1865, 1866, and 1869 Atlantic cables, in order to allow the necessary amount of slack to run out, and to evade possible irregularities at the bottom, it was necessary to reduce the strain upon the cable to one-third or one-fourth the weight of the length of cable that would have hung had the line been perpendicular to the bottom. There is, indeed, no difficulty in the mechanical operation of laying a cable, provided it comes out of the tank without getting entangled, or getting foul of its machinery, and the amount of strain is so small that no risk arises upon that score, and no trouble need be experienced. But where the difficulty is experienced is when, owing to any cause, you have to haul back the cable. The cable has now to be lifted sideways, against the water, and the resistance of doing that is so great that, in 1869, when pulling up a cable from a depth of 2,400 fathoms, more than twice the strain due to the weight of the cable had to be applied, and then the cable did not come in so fast as half a mile an hour. There is the difficulty, that you have not only to lift it from the bottom, but to lift it broadside upwards against the water, and, consequently, you are obliged, in deep water, to lift the cable very slowly, or you are bound to break it.

Another portion of the paper refers to a subject in which I am

personally interested; that is, in the testing of faults. As early as 1847, in the London street wires, I found it shortened the operation of removing faults very much to be able to indicate by the measurement of the strengths of current what was the length of line under test. The faults we had were principally caused by the solder of the joints running in and forming contact with one or more of the copper wires, thus causing what is technically called "metallic" contact. It then became necessary to measure the resistance between the wires and the leaden tube which was then used. That, I think, was the first practical test for faults, and it was a daily practice with me for months; still, there can be no question that the first algebraical formulæ showing how to get at the position of a fault were published by Dr. Siemens. The paper containing this was printed in March 1852, and afterwards in German in Poggendorf's Annals. In that paper he gave a method which was practicable, provided you had, as he required, two comparable galvanometers—a rather difficult thing to find in those days. He has, however, it appears, since modified this method very considerably. It was about the year 1857 I published a formula, unaware of the one for getting the position of faults in wires by testing from one end; and at that time I thought this was the first attempt to indicate the position of a fault by this means. It was, however, original in this respect,—that, while Dr. Siemens required two operations, with comparable instruments at each end, in my case only one operation was required at one end; and that method is, on that account, very often very convenient.

In 1857 Messrs. Glass, Elliot, and Co. made a cable for the Electric and International Telegraph Company, which was submerged between England and Holland. During the submersion of this cable a fault appeared in No. 2 wire. The vessel was stopped: tests were made. I said, after some hesitation, the fault must be on board. Tests being a new thing then to Glass, Elliot, and Co., and there being no proper picking-up apparatus on board, it was decided to lay the cable and repair it afterwards. The cable was accordingly laid, but before we reached the other side a fault appeared in No. 4 wire, although of a smaller character. The method of testing to which I have just referred answered very well

for obtaining the position of the fault in No. 2 wire, which was a metallic fault, one conducting wire being in contact with the other wire: this was shown by the fact that there was no polarization from this fault, but all attempts to get at the distance of the fault in No. 4 wire, after two days' labour, entirely failed. We could get no nearer to it than to within a distance of about thirty miles, because the resistance of the fault was constantly varying. When I came back to London I set myself thinking over this, and the result was the loop test. I went back, tried it, and with good results, for resistances were obtained which did not vary a mile. The loop test, which is familiar to you all, I published in the year 1858.

In the following year a trial of the value of the loop test took place between Glass, Elliot, and Co., and Mr. Boswell, who was in the service of Mr. Newall, it having transpired that a man in the employ of Mr. Newall had driven a nail into the cable for the purpose of destroying it. On coming to the fault in the No. 4 wire I found that several attempts had been made to injure the cable, although they had resulted in only one minute fault.

That is the first instance I know of endeavouring to meet and successfully meeting the difficulty of dealing with a variable fault. Dr. Siemens seems to claim that for himself, but inasmuch as this was published first in my patent of 1858, and again in 1859, before the Committee of the Privy Council, and again in 1860 before the British Association, I take this opportunity of claiming for myself both these two methods to which I have referred, and I believe I am fully entitled so to do.

I have still a few more remarks to make, but, as the time for closing the meeting is already past, I shall defer these until our next meeting.

The discussion was then adjourned.

The following Candidates were balloted for and declared duly elected:—

FOREIGN MEMBERS:

H. A. M. Reeder
I. de Jager
G. Blocklinis
R. von Eldik
I. C. Evers
M. C. de Graaffe

MEMBERS:—

Wilhelm Kieser
Andrew T. Maginnity
H. Izaak Walton
William Ladd
H. M. O'Kelly

ASSOCIATES:

Frank Fisher
George W. Frodsham
George C. Bompas
William Gurdon
John D. Barry
S. Butcher
J. Phillips, R.E.

The Meeting then adjourned.

The Forty-fourth Ordinary General Meeting was held on Wednesday, the 23rd February, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The PRESIDENT said: At the last meeting the paper read was by Dr. Werner Siemens, "On the Theory of Submerging and Testing Submarine Telegraphs." Mr. Varley was on his legs when the time arrived for closing. I shall therefore call upon Mr. Varley to continue the remarks he has to offer; but before doing so I may be allowed to supply an omission in a matter of date and history which occurred in Mr. Varley's remarks. I am not surprised at an item or so escaping notice when going into dates, among which I have myself been floundering for some time, and have been feeling the difficulty of making them complete and accurate. With regard to the early history of gutta-percha wire in this country, on referring to the abridged list of patents taken out from 1627 to 1857, published by the Commissioners of Patents, I find the first three patents taken out for telegraph wires covered with gutta-percha are these, viz. in 1848, April 27th, by W. H. Barlow and Thomas Forster; the Barlow part having reference chiefly to instruments, and the Forster part to gutta-percha covered wire. On the following day, that is on the 28th of April, I sent to Mr. Forster two miles of wire to cover with gutta-percha. His plan was to pass two strips of gutta-percha between grooved rollers heated by steam, with eight or ten wires between the strips; and the two bands of gutta-percha were thus pressed together around the wires, and the covered wires were nearly, if not completely, cut apart by the process. The patent referred to by Mr. Varley as the first patent——

Mr. VARLEY: I beg your pardon; it was Mr. Willoughby Smith and not I who made those remarks.

The PRESIDENT: I beg Mr Varley's pardon. The second patent for gutta-percha covered wire was by Mr. Lewis Ricardo, and dated

September 4th, 1848, and the third was dated April 23rd, 1850, by Mr. Siemens. Those were, as far as I can ascertain, the first patents that were taken out in this country for covering wires with gutta-percha.

Mr. Varley, F.R.S.: I believe on the last occasion I omitted to mention that the first real attempt at making and laying a submarine cable was that of the Submarine Telegraph Company, and I think there can be no doubt whatever that Mr. Jacob Brett and his confrères were the people who first of all had the courage to find the money and had the boldness to make a cable and lay it successfully. In speaking of the printed copy of this paper I mentioned in passing the method of testing as being that of Mr. Latimer Clark. When the Atlantic Cable failed in 1865 Sir W. Thomson and I were engaged by the Telegraph Construction and Maintenance Company to advise them as to the best means to be adopted for testing the cable of 1866. We recommended the system introduced by Mr. Willoughby Smith for measuring currents at the receiving end by interposing great resistance at that end, which did not interfere sensibly with the insulation of the cable but yet allowed the receiving station to receive from time to time signals from the ship. At that time we prepared what has ever since been called the potential method, that was the method of measuring the potential first at the junction between the testing battery and the resistance; secondly, at the junction between the cable and the resistance; and lastly, at the different ends. This method will be found in the archives of the Telegraph Construction and Maintenance Company about November or December 1865.

Mr. Lawes and Mr. Willoughby Smith recommended, instead of the use of a galvanometer, the use of condensers, which should be applied to these points and simultaneously discharged through a galvanometer, to measure this potential. Sir William Thomson was in favour of the electrometer, whilst I was in favour of the galvanometer; but it was, in effect, all the same thing, merely under different colours. On pages 17 to 20 in the printed paper Dr. Siemens produces a method which I cannot see very well, which is shown in figure 2: it consists of applying such a battery

at this end and such a battery at that end as shall produce equal potential at either terminus. Now, if you had your cable in the room, and your fault was a metallic arc, so that there was no variation of the fault, you might apply that method with success. I discussed that method in 1869 with Mr. Charles Hockin, and later with Sir William Thomson. I endeavoured to put it in operation on the French Atlantic cable in 1872, but owing to the continued variation of the earth-currents it was impossible to get any reading that was of value. In fact, all ordinary methods applied to that particular fault (where the resistance was from 25,000 to 30,000 ohms) were useless for finding it out. They were, indeed, such, that at one time they might give the fault at San Francisco, and at another at St. Petersburg; therefore, it was necessary to adopt another method to get over that, and this is the method which I adopted with complete success. I sent out a signal to St. Pierre to charge his battery with sawdust mixed with oxide of zinc, and solution of zinc sulphate for the zinc cells, and to take care that no copper got into those cells. The zinc plates were amalgamated every morning. The copper cells were charged with a solution of neutral sulphate of copper. By this means you get a battery, the different cells of which will not vary much, and, this being a battery of forty cells, they gave a very uniform result, so that we were able at each station to get a sufficiently accurate measure of potential by means of this battery and resistance apparatus. [Illustrates.]

The result was, that, although the fault varied, yet we determined the position of it within three miles, not by any one test but by a mean of a number of tests, for they varied very much even with all precautions. It was the more important to determine accurately the position of this fault, because, had it been three miles further off instead of being in 230 fathoms of water, it would have been in 2,000. Now, in this method, with constantly-shifting earth-currents between the fault at one end and the other, it is impossible ever to adjust the batteries on either side so as to get reliable readings. As I have said before, the first person to deal with variable earth-readings was myself; but the subject was one which occupied the attention of other persons, and, amongst others,

Mr. Murray produced (in 1869) a method which can be used in the factory with great advantage when you can get at both ends of the line.

The last remark I have to make is that submarine cables are undoubtedly of English origin and design. The first real practicable cable was made under the auspices of Mr. Jacob Brett. It was English capital that led to the construction of that cable: it was English capital that led to the construction of the first long cable: and I am not going too far when I say that it was due to English pluck and to English perseverance that we have any cables at all. To this I will further add that the electricians of England were the first to ascertain the positions of faults by testing, although I admit that Dr. Werner Siemens, who is perhaps the greatest telegraph authority on the Continent, was the first person to produce a formula for ascertaining the distance of a fault.

Mr. C. W. Siemens, F.R.S.: I thoroughly concur with the concluding remarks of the last speaker that submarine telegraphs are specifically English enterprises. I might go further, and say every submarine cable which is now working is, almost without exception, the produce of this country, and has been shipped from the Thames.

With regard to my brother's paper, it was remarked on the last occasion that it is essentially a theoretical paper. It was intended to be such, and I am glad it has elicited such able remarks as those which have fallen from Mr. Varley. We have all heard of "Varley's fault" in the French Atlantic Cable, and I have been glad to hear the method employed for finding the position of that fault with such accuracy. The difficulty, and the only difficulty in the way of determining the position of such faults, is the earth currents, and Mr. Varley has dealt with great success in this instance with those disturbing influences, and has worked upon a different method to that pursued by my brother, who wished to reduce the effect of polarisation at the point of the fault to a minimum by eliminating for the time being the earth current, and taking the earth current and battery current together, producing an equilibrium at the point of the fault. That is a method which I think is well worthy of the consideration of practical telegraphists,

but there are more roads than one leading to Rome, as is proved by the success of Mr. Varley's method.

Regarding the early history of gutta-percha which was discussed at the last meeting of the Society I wish to make a few remarks. I may say I stood on the threshold when gutta-percha was first introduced into this country. This was I believe in the winter of 1844-5, and not in 1843 as stated by Mr. Willoughby Smith, because I recollect well seeing the first specimen of gutta-percha exhibited at the Society of Arts, I think by Mr. Montgomerie. At that time I was young and enthusiastic, and I begged Mr. Montgomerie to give me a piece of this wonderful stuff, the contemplated application of which did not seem to go beyond the formation of whips and similar articles. He was kind enough to give me a piece, which I forwarded to my brother Dr. Werner Siemens, who was at that time an officer in the Prussian service, and a junior member of a Commission appointed to report upon the feasibility of telegraphs. He had the idea that the wires should be covered with india-rubber and laid under ground, and I sent him this piece of gutta-percha in order that he might try whether it was not superior to india-rubber for insulation purposes. He did so, and after some time, having procured for him at his request a further supply, he made experiments, and in the course of about twelve months he proposed to the Prussian Government the use of gutta-percha for insulating the telegraphic line wire. In the first place he tried to unite two strips of gutta-percha round the wire, and the line from Berlin to Grossbehren was laid in 1846 in that way. It was soon found, however, that the moisture penetrated to the wires, and this led my brother to design a machine which is still in existence and was exhibited at Vienna, and which is very similar to that used for maccaroni making. This machine was designed in 1847, and in the early part of 1848 some hundreds of miles and in 1849 some thousands of miles of wires made by means of it were laid in Germany. My brother did not at that time take out a patent for his machine because he was in the Government service, and as it had been done partly on behalf of the Government it had become public to a great extent: the patent referred to as having been taken out by him in 1850 will be found

to embrace only some improvements in this machine. Hence it is an undoubted fact that gutta-percha was applied to the insulation of wire in Germany several years before the patents mentioned by the President this evening as having been taken out in 1848. I should correct myself. The patents taken out in England in 1848 were for covering the wire between strips of gutta-percha, a method which had been tried by my brother in Germany in 1846; but the covering of gutta-percha by means of a machine working on the principle of a lead piping or maccaroni machine was, I think, not adopted by the Gutta-percha Company until 1850. Therefore, although submarine telegraphy is decidedly an English enterprise it must be admitted that much has also been effected abroad to bring appliances to their present state of perfection.

Another remark I think fell from Mr. Varley with reference to water tanks on board vessels, and he implied that my brother claimed the introduction of those tanks. If he refers to the paper, Mr. Varley will find that is not the case. He does not claim the tanks, but says they were introduced in England. But it so happens I have had a great deal to do myself with the employment of these tanks. Whether I was absolutely the first to broach the idea or not I will not say. It might have occurred to several, but may I say this, that in 1859, when the Rangoon and Singapore cable was carried out for the Board of Trade I was employed to test that cable, and I strongly urged upon the Government the construction of water-tight tanks on board the steamship "British Queen." The matter was referred by Messrs. Glass and Elliott to the constructors of the ship at Newcastle, who wrote a letter to the Board of Trade stating that they thought it impracticable, that water-tight tanks constructed on board ship would inevitably fail on account of the natural motions of the ship, and my recommendation was negatived. This was, perhaps, fortunate, because it gave rise to the first application of the resistance thermometer for ascertaining the fact that a cable is subject to spontaneous generation of heat when coiled in a dry tank, and of proving the absolute necessity of water-tight tanks, which, as is well-known, have been in use ever since.

I should like to make a few remarks regarding an observation

that occurs in the paper where my name has been mentioned in connection with a method of finding the depth of water below the ship in paying out a cable, and as this is a matter of some interest I will explain more fully in what this method consists. It was used first, I may say, by myself in laying the first section (the shore end) of the Direct United States Cable, the other section having been laid partly by my brother Mr. Carl Siemens, and partly by Mr. Loeffler. We passed across considerable depths of water. The first cable laid was laid upon the solid bottom of the sea. The second cable was laid very much to the south of the first, so as to leave sufficient distance between the two cables. We did not know the depth of water between the shore and the extreme end of this headland (*illustrating on the board*); and as the cable was a heavy one it was important to know the depth. Most of you know that in paying out a cable from a drum there is really no direct indication of the depth of sea below the ship. The strain which is applied is meant to be such as to balance the weight of the cable from the ship down to the bottom of the sea; but if the depth is not known it is difficult to say what the retarding force should be. By applying too much you get a tight cable; with too little, much cable is lost in depths which are considerable. The motion of the ship through the water is not a sufficient criterion, because you may be moving with the water at a considerable rate. But there is, nevertheless, a method which the practical cable-layer may resort to for finding out whether he is paying out the proper amount of slack or not, and by the same means ascertain the depth of water below. Assume that the cable runs out over the drum, with a dynamometer attached to it, at the rate of five knots an hour, and the strain is one ton. This may be a proper amount of cable to be paid out upon the ground; but it may be the ship is going only three knots an hour over the ground instead of five. To ascertain whether it is so or not—the strain being twenty cwt. on the dynamometer—increase the strain by another cwt., and then carefully note the number of revolutions of the wheel per minute. If the increase of one cwt. has no effect upon the number of revolutions of the paying-out drum, then it is pretty sure that unnecessary slack is not being paid out; but if the increase of one cwt. on the dynamometer

causes the number of revolutions to fall sensibly—say from fifteen or sixteen revolutions per minute to fourteen—then too much slack is being paid out, and the weight should be increased. If the case is doubtful I would put on a considerable amount, say three or four cwt. This would (if a great deal of slack is being paid out) stop the break-wheel, and the ship will pass over the ground without paying.

The President: In continuation of this discussion we have a communication from Mr. Sabine, with some extracts from the correspondence of the late Sir Charles Wheatstone, having reference to his early views and his experiments with submarine cables; but it is possible, as we have been talking of the early history of gutta-percha wires, that Mr. Forster, who is present this evening, might feel disposed to say a few words as to his early experience of the insulation of wires with gutta-percha in England.

Mr. Forster: I did not expect to be called upon to speak, but I can tell you the original mode adopted, and why it did not succeed effectively. The plan of carrying the thing out was to cleanse the gutta-percha, which was done first by putting it through a colander and washing and masticating it between two hot rollers, then putting it between two pairs of rollers one above the other, with guides at the sides, so that the gutta-percha was put into the top pair and into the lower, and both, acted upon by the same motion, brought out a strip each about four inches wide. These were taken to a pair of fluted rollers made to carry twelve wires through the flutes, one strip being placed over and the other under the wire; the band of gutta-percha and wire came out at the other end as a complete belt, and the edges of the flutes nearly cut through. But you are aware that gutta-percha is charged with a large amount of woody substances, and it is difficult to get rid of them thoroughly; and the result was in the first instance numerous faults in the wires when they were tested. After the difficulties in connection with that were overcome, wires were laid and used. I believe the whole of the wires through the tunnels on the South Eastern Railway were so laid, and it was found to be of great advantage. But very shortly after that a great improvement was made by the Gutta-percha Company, I think under the auspices of

Mr. Beverley, and I am of opinion that it was called his patent at the time. That had this advantage over my plan. It was a cylinder with a small box at the side, the one in fact now used. The cylinder was kept hot by a steam jacket, the gutta-percha was put into it, a piston was driven down, and at the extreme end was a small box with a trough of water, and a thin coating of gutta-percha was put upon the wires each time of passing through the cylinder. The wires had several coatings of gutta-percha put on them, so that the faults of one coating were remedied by that which was put on afterwards; and this made a very perfect article, both for use and in appearance. Prior to that the Chairman of the Electric Telegraph Company, who was of course acquainted with the plan I adopted, patented a mode of keeping the wires in one band of gutta-percha instead of separately. But it was found that the union of the two strips of gutta-percha was not so perfect as was anticipated, and consequently the plan did not answer well for telegraphic purposes.

The SECRETARY then read the following communication from Mr. Robert Sabine:

25, Cumberland Terrace, Regent's Park, N.W.
15th February, **1876.**

To the Secretary of the Society of Telegraph Engineers.

DEAR SIR,—As the Members of the Society of Telegraph Engineers have at the moment under discussion the subject of Submarine Telegraphy, it has been suggested to me that it might be interesting to them, from a historical point of view, to inspect the drawings made to illustrate the earliest plan on record—that suggested by the late Professor Wheatstone—for the establishment of a submarine telegraph between France and England.

I have therefore sent you these drawings, and I shall be much obliged if you will kindly have them placed upon the table for the inspection of the Members at the next meeting.

From the perusal of old letters which have recently come into my possession, I find that a submarine electric telegraph was, as early as 1837, a theme upon which Professor Wheatstone was greatly interested, and upon the preliminary details of which he appears to have spent a good deal of time.

The earliest printed mention of this scheme is to be found in the fifth Railway Report of the Select Committee of the House of Commons. When under examination before this Committee, on the 6th of February, 1840, Professor Wheatstone gave evidence as to his opinion of the practicability of establishing an electric communication by means of a cable between Dover and Calais.

On reference to *Le Fanal*, a Brussels paper of the 30th September, 1840, you will find it stated that: "M. Wheatstone pense qu'il est possible de communiquer avec son appareil entre Douvres et Calais; il répète en ce moment ses expériences à l'Observatoire de Bruxelles, en presence de plusieurs savans littérateurs."

And in the *Bulletin de l'Academie Royale de Bruxelles*, for October 7th, 1840, you will find a notice of Professor Wheatstone's new telegraph instruments, written by Professor Quetelet, in which it is stated: "On sera sans doute charmé d'apprendre que l'auteur a trouvé le moyen de transmettre les signaux entre l'Angleterre et la Belgique, malgré l'obstacle de la mer. Son voyage se rattachait en partie à cette importante opération, qui mettrait l'Angleterre en rapport immédiat avec notre pays, la France, la Hollande, l'Allemagne, et même la Russie."

After making his experiments in the Observatory at Brussels, Professor Wheatstone appears to have returned to England and to have occupied himself diligently with the preparation of the two detailed plans which I have the pleasure of sending you. I find from a note in his handwriting that they were completed in October 1840, and were exhibited to a great number of visitors at King's College.

Sheet I. shows the method of insulating and making the cable, and how it is to be put on board the laying-ship. It contains:

(1) A section, end-view, and plan of the apparatus for wrapping the copper conductor with its insulating cord;

(2) The elevation and end-view of a machine for simultaneously covering seven such wires to form a cable;

(3) A section and plan of a machine for binding the seven covered wires with an outer serving of cord so as to combine them into a cable; and,

(4) Along the bottom of the drawing how the cable, in its various stages, is to be passed through baths of insulating material, and how it finally reaches the ship.

SHEET II. shows the proposed route of this cable, and the methods of laying, joining, and under-running. It contains:

(1) A section and chart * of the channel between the South Foreland and Cape Grisnez;

(2) The cable-barge being towed by a steamer and paying out the cable;

(3) A section and plan of the stern end of the cable-barge, showing three of the drums on which the cable was to be sent to sea;

(4) The method of connecting the end of the cable from one drum to that on another;

(5) The steamer under-running to find a fault; and finally,

(6) A section and perspective of a piece of the proposed cable.

These drawings were executed for Professor Wheatstone by a Polish draughtsman, named Lutowski, who was at the time in his employ.

I find a MS. article in his handwriting, entitled, "On a means of establishing an Electric Telegraph between the coasts of England and France," in which occurs the following passage: "Each wire should form the core of a rope line well saturated with boiled tar, and all the lines be made into a rope prepared in the same manner." This gives an idea of the kind of insulation contemplated in 1840.

In the year following (1841), Professor Wheatstone appears to have gone with his scheme and these drawings to Paris. During his stay there, he says, in a MS. note, that he let Mr. J. Joseph Silbermann take tracings of the drawings.†

Professor Wheatstone does not appear to have confined his cable scheme to joining France and England; for towards the end of

* "Captain White's chart of the English Channel, given me by Captain Beaufort." —MS. note.

† "These tracings I find, from a letter, dated 22 July, 1855, from Mr. Silbermann (College de France), were lent by him subsequently to M. Pouillet, who deposited them in the Conservatoire des Arts et Metiers."

1840 I find him in correspondence with Captain Beaufort of the Admiralty, who, in a letter dated 19th December, 1840, encloses the depths between Portsmouth and Gosport, and between Portpatrick and Donaghadee, and promises further aid in the matter.

But the Channel telegraph still appears to have been the ultimate object, for I find a letter dated April 5, 1843, from his solicitor, Mr. Richardson, in which, referring to some documents then in course of preparation, he says: "I have introduced your right to establish telegraph communication between France and England."

A preliminary experiment on a less ambitious scale seems however to have been wisely determined on. And, in the month of September 1844, there is a memorandum in the handwriting of Professor Wheatstone, that in company with a Mr. J. D. Llewellyn he made experiments on submerged insulated wires in Swansea Bay. I find a letter from Mr. J. D. Llewellyn in which he says that they "went out in a boat making communication with the Mumble-head lighthouse and testing the efficiency of various kinds of insulation." . . . . "The old lighthouse-keeper had been an assistant of some sort to W. Snow Harris, and took great interest in what was going on." . . . . . . "he was an intelligent man, and gave every help in his power by reading the signals and communicating with us in the boat." . . . . "made trials in deep water and among wet seaweed on the shore."*

Professor Wheatstone after his experience in Swansea Bay returned with renewed vigour to his original Channel project. Dated September 1845, I find the following interesting letter from him to Captain Beaufort, R.N., Admiralty:

"20, Conduit Street, Sept. 23rd, 1845.

My dear Sir,—I am now preparing some experiments to test the practicability of establishing an electric telegraph across the Channel from Dover to Calais. To this end you were so kind as to give me several years ago the necessary charts and other valuable information. You will add to the obligation I then

* J. Dillwyn Llewellyn, Penllergare, Swansea, Oct. 24th, 1866. The contents of this letter are confirmed by a letter from his brother, Mr. L. L. Dillwyn, Hendrefoden, Swansea, 23rd October, 1866, who says, "I well remember you making the experiments to which you allude when you were staying with my father."

incurred by assisting me in the solution of any of the following questions:

"1. Will the current at any part of the passage across the Channel have any effect in displacing a rod or tube of lead, being in diameter from a quarter to half an inch?

"2. What would be the effect of a ground-swell on such a rod or tube?

"3. What is the depth of the sand above the chalk in various parts of the passage?

"4. How much time would elapse before the rod or tube would become imbedded in the sand?

"5. Is any danger to the tube to be apprehended from the anchoring of vessels, the dredging of fishermen, or the raking of smugglers; and, if so, how is such danger to be avoided?

"6. What is the effect of the long-continued action of sea-water upon lead?

"7. Is there any lighthouse on the coast of England, or an island, or rock, within a mile from the land, where a telegraphic communication to the shore would be useful? Or is there any guard-ship for which it would be an object to transmit instantaneous intelligence to the shore?

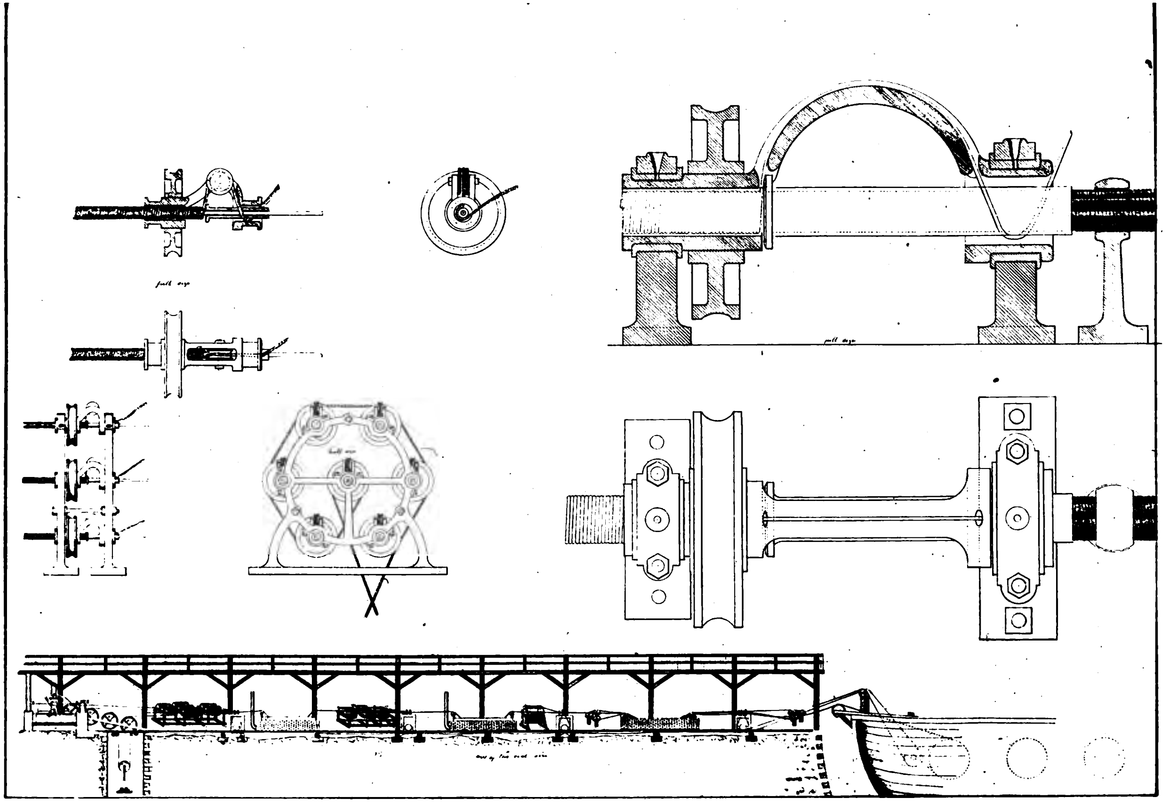
"I remain &c.,

"C. Wheatstone."

The allusion to lead tube in some of these questions explains the meaning of a long bill which I find, dated between December 1845 and May 1846, from Mr. W. H. Darker, of 9, Paradise Street, Lambeth, for making experiments "to enclose a copper wire insulated with worsted and marine glue in a lead pipe." And on 11th August, 1846, a bill from Mr. H. Mapple for "making nine thousand feet of tube-protected wire;" enumerating the materials as "lead, copper wire, marine glue, and cotton."

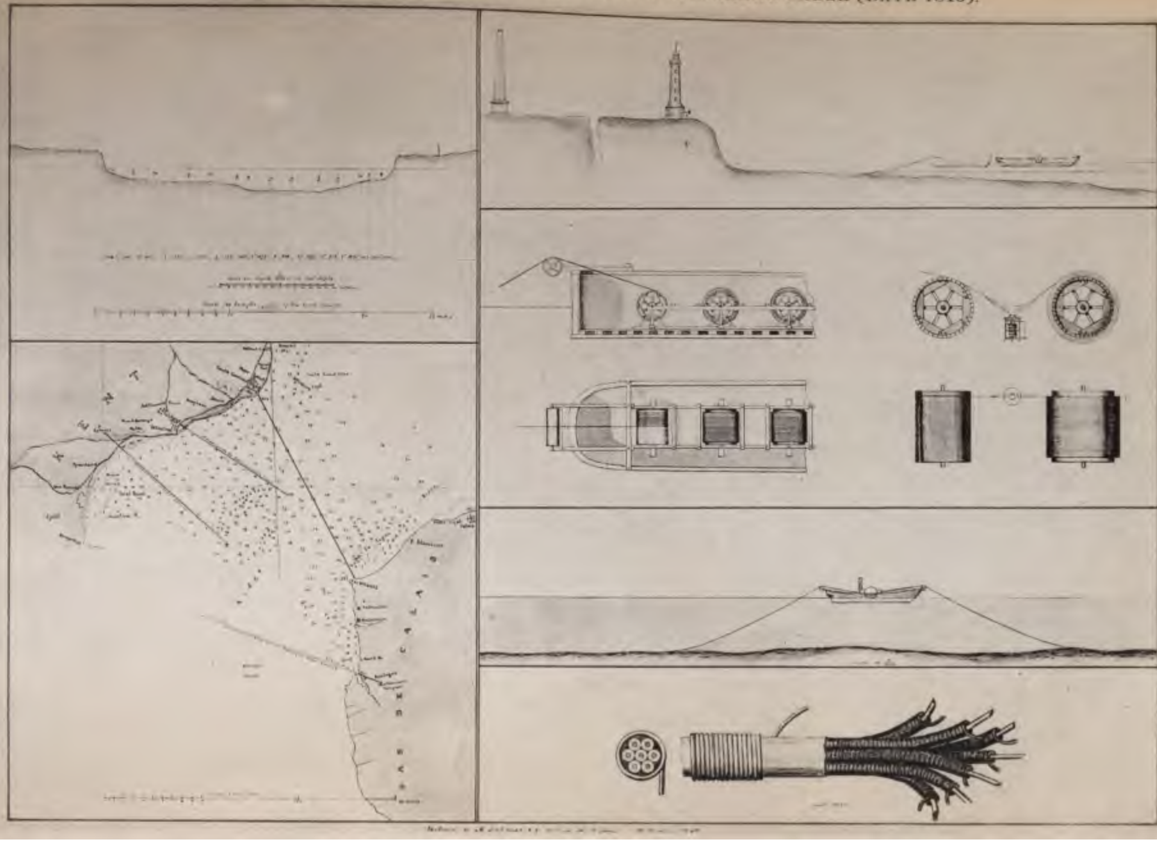
Between June and August 1846, I find letters from Mr. W. H. Hatcher, Engineer to the Electric Telegraph Company, with regard to a proposed line of telegraph in lead tube for crossing the harbour at Portsmouth, for which the above was probably intended.

It may be interesting to the members of the Society to know



SHEET I. The method of insulating and making the cable, and putting it on board the laying barge

SIR C. WHEATSTONE'S ORIGINAL PLANS FOR A SUBMARINE CABLE (DATE 1840).



that as early as 1845 Professor Wheatstone contemplated employing gutta-percha in the construction of his proposed cables; but how he proposed to apply it is not clear. In 1845 I find him in correspondence on the subject with a Mr. Edward Solly of Bedford Row, who promises to procure some for him; and on October 18th of that year I find a letter from a Mr. J. S. Lister, lamenting "that there is no gutta-percha in the market, that the last parcel was purchased by Mr. Hancock of Charing Cross, and that its value is about 1s. per lb., that the supplies expected for the next two years have been bought up by some party who has a patent for the use of it in an atmospheric Railway."

So far as I have had time to examine Professor Wheatstone's old papers, I have not found much else than the above which bears historically upon the subject, but my search is not yet complete.

The practical and engineering details which are embodied in these plans and suggestions appear of course to be very crude, and the electrical part to be very insufficient when judged by the measure of our present knowledge; but it is necessary to remember that this pioneering work was all in process of creation years before such a thing as a real submarine cable was seriously attempted, and the author of them had no experience of any analogous operation to guide him. Viewed in this light, I feel sure that the Members will regard these two drawings as most interesting relics in the earliest history of submarine telegraphy, and that the length of this letter will be pardoned in consequence.

I am, dear Sir,

Yours very truly,

ROBERT SABINE.

NOTE.—The two accompanying illustrations have been done from negatives taken directly from fac-similes of the original drawings by the photo-lithographic process. The dimensions of the original sheets are 28″ × 20″.

Mr. F. C. WEBB: This discussion hitherto has formed an historical abstract of the development of submarine telegraphy, and I have no doubt there are many gentlemen present who well recollect many points which have occurred during the last twenty-five years, and I think it is our duty to place on record such facts as we may be in possession of, and which we may think of importance in an historical point of view.

With regard to the early days of practical submarine cables, I recollect and was present at the laying from the "Goliath" of the first Dover and Cape Grisnez unprotected gutta-percha insulated wire, which may be regarded rather as an experiment. Subsequently the "Blazer" payed out the existing four-wire Dover and Calais cable, which has lasted twenty-five years. That was also an experiment, to a great extent, in the paying out work, for hardly any machinery was employed in the operation, and the first cables, I think, which were laid by anything like mechanical means were the Holyhead and Howth and the Dover and Ostend lines, laid by Messrs. Newall. I was not present on those occasions, but afterwards examined the machinery by which they were laid. I believe that was the first time the drum-break was used. Now we have the Appold break added, instead of the ordinary strap and lever, but the drum now used is the same as that which was introduced by Messrs. Newall in 1853. Of course if it had been merely the fact that Mr. Newall had used the drum-break for the first time in those early days, and afterwards there were improvements on it, there would be no such very great credit in it, because nobody else had a chance of trying his talents in the laying of cables, but when we come to the Atlantic cable of 1857 another kind of break was

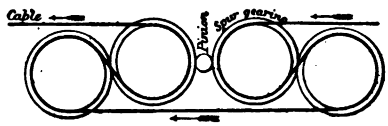


Fig. 1.

adopted, which was like that shown in fig. 1, and was used as a supposed improvement on that of Newall.

In the next year there was a Select Committee appointed, and

the whole Institution of Civil Engineers was invited to give an opinion on the best form of break, which resulted in a break with two wheels, with grooves in them, as shown in fig. 2, the cable passing four times over them. These wheels were not geared for paying out, and for the first time the Appold break, now so well known, was then applied.

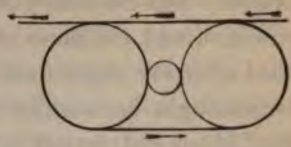


Fig. 2.

Now in a break we require two things: one is the means of producing the friction, and the other is the means of holding the portion back to which you apply the friction. In the Appold break (fig. 3) the way in which the friction is caused by a screw adjusted by hand at A, and the weight is applied by weights at the end of this leverage (*pointing*).

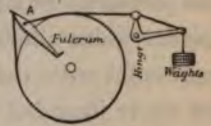


Fig. 3.

But this break (the four-fold grooved wheels), though used on the Atlantic cable of 1858 successfully, has I believe never been used since, and we have come back to the old drum-break of Newall, in some cases with the Appold break, but in others with only the ordinary strap and lever, therefore it appears to me it redounds to the credit of Mr. Newall that what was used in the early days of submarine cables, and then abandoned, was taken up again in later times.

With regard to the historical portion of the subject the Spezzia and Corsica cable was the first cable in deep water which was laid by Mr. Brett, but it should be recollected he took with him Mr. Thompson, one of Mr. Newall's head assistants. For some portions of the distance there was a depth of 500 fathoms of water. It was a heavy cable, carrying seven wires, and weighing about seven

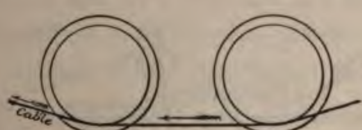


Fig. 4.

Fig. 5.

tons per mile, and they had to take extra precaution on account of the great weight. They used two ordinary drum breaks, each with double

straps, with five turns round each drum. (Figs. 4 and 5.) It must have been a very powerful break and just the thing for laying such a cable. But the next year Mr. Brett did not take Mr. Thompson with him, and the same machinery for a six-wire cable absolutely failed after two starts from the land and about eighty miles of cable were brought back whilst sixty miles of cable were picked up by Mr. Liddell and myself in the Elba, and that was the first heavy cable picked up from deep water. On a second occasion a cable which reached within a short distance of the Island of Galita was lost, as they had no buoys: a portion of it was recovered by the expedition that picked up the sixty miles of heavy cable. In the case of the Bona-Cagliari cable alluded to in the paper it is mentioned that water-tight tanks were used. That is not correct. They were iron tanks but never intended to be water-tight at that time, and I think Mr. C. W. Siemens is right in taking credit for being the first to recommend the adoption of the water-tight tanks. The tanks used in 1857 for the Bona line were iron tanks but not intended to hold water. That cable cannot be said to be an absolute success, although no doubt it was the first cable that was eventually carried from land to land, but it ran short thirty miles from Chia were it was to be laid, and my first work with Mr. Newall was to go out and pick up the end and complete the cable for the distance of thirty miles. We went out in the "Blazer" tug from Marseilles; we had no picking-up gear, not even a bow sheave, and I rigged up a temporary bow sheave. When the cable ran short they had spliced on ten miles of single wire cable and had run this within ten miles of Cape Spartavento. This we grappled for and then picked up by hand till we came to the splice. There is one point with regard to that

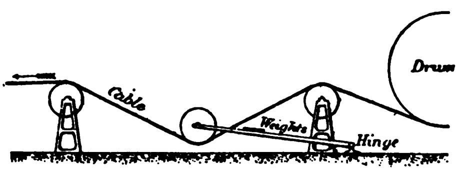


Fig. 6.

cable which deserves historical notice. It was the first cable on which a dynamometer was used, and I think Dr. Werner Siemens

can claim that he was the first person to recommend, whilst Mr. Newall was the first to adopt, the dynamometer. It was a very rough kind of dynamometer, but answered the purpose. It is shown in fig. 6. I was not myself engaged in the laying of the Bona cable across the deep water but only at the completion of it, as I have mentioned. I was afterwards on the Malta and Corfu cable in the same ship, the Elba, and that was the first time I saw and used the dynamometer that I have described. This form of dynamometer was improved. I speak only from hearsay, but I believe in the case of the first Red Sea cable Mr. Newall adopted this plan (fig. 7). I am sorry Mr. Fleeming Jenkin is not here, as he was on board at the time of the working of this machine. It

Fig. 7.

was a kind of self-acting break and dynamometer combined. The dynamometer most generally used at present was first employed in the 1858 Atlantic.

The question which Dr. Werner Siemens asks in his paper, as to whether the formulæ of Messrs. Longridge and Brooks is correct or not, I am not able to answer, as I do not feel myself competent to enter into the mathematical analysis; but I think if there was anything materially wrong in Messrs. Longridge and Brooks's calculation there would have been something to point out the discrepancy before now, because many cables have been laid across the Atlantic and we have never heard anything which disagrees with the results pointed out by Longridge and Brooks. It is true very few persons may have gone into the calculation to prove how the actual strain upon a cable corresponds with that given by the formula, for even to do this we should require to know the coefficient of the longitudinal friction, and this is not often tried. Therefore how we are to test the correctness of that calculation I do not know, because nobody takes the trouble to go into it, and whether the longitudinal coefficient is as the square or directly as the velo-

city there seems to be no means of ascertaining. Cables are laid without these calculations being made, and there appears to be no means of showing how far they are correct in a minute point; but taken generally we know that they agree as regards the relation of speed, strain, &c., with practice. But there are points in Mr. Longridge's paper which any engineer who has to lay cables would do well to think of, and there are some points which I believe involve very serious considerations. There is one point in particular. He shows that if the ship which is paying out the cable, which descends in a straight line, inclined to the horizon, is stopped suddenly and remains stationary, the cable will assume a catenary (fig. 8), and if the cable has been paid out without slack a great strain will come on it.

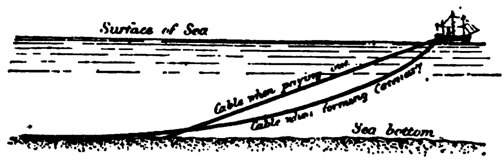


Fig. 8.

In paying out the old Atlantic cable at the rate of 6 feet per second the strain would be about 148 cwt., whereas the cable itself would only stand about 85 cwt. Of course in practice the ship would be put astern so as to prevent this strain. But there is another case where this problem comes into play which is far more serious. Supposing we have to deal with ground at the bottom of the sea like that shown in fig. 9 (and this is a matter pointed out by the French writers, but never by English), as the cable sinks in a straight line very slightly inclined to the horizon, in paying out when the cable suddenly touches the high point here (*pointing to* A), it will soon be moored by the cable that sinks to the ground on the ship's side of the point A, and if this occurs before the portion A B has had time to sink and drag sufficient slack back over the point A, so as to allow it to take up a catenary of small strain, the cable may be left with a flat catenary, giving considerable strain on the cable, and no doubt that would be the case where the cable is even laid with some slack but laid with too small a per-centage of slack, and this may be the cause of some of the failures. For

instance, in the Atlantic cable of the present day there are two faults which cannot be accounted for, and it is quite possible in time that may be found to be the reason of their breaking there. [Mr. Webb further illustrated this point by drawings on the board. Fig. 9.]

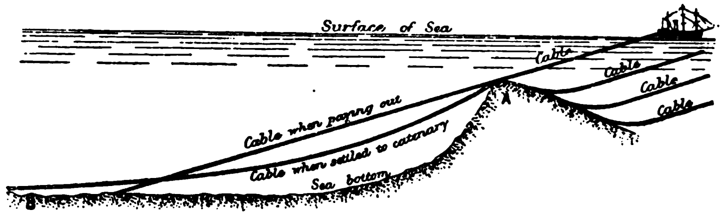


Fig. 9.

With reference to the observations of Mr. Siemens this evening as to his method of testing the depth of water below, I may state I adopted that plan in the laying the Direct Spanish cable at points where we had doubts about the soundings. I put on more break power to ascertain whether the slack decreased, and, finding it did not, I knew we were in shallow water and acted accordingly.

There is only one other point to which I will refer, that is as to the drawing No. 4, with regard to testing faults. I have myself suggested this test in my paper read before the Institution of Civil Engineers in 1858, and indeed to some extent used the principle roughly as stated in my paper. I do not see that the test is complete as given by Mr. Siemens, but it requires consideration whether something could not be done. There must be some means of finding out, after having taken the resistance of the line and the return current, the resistance of the end. I do not see that that is solved in the paper. The formula gives two-thirds of the whole charge as going out here, at the near end, and one-third out here, the far end. That does not solve the question, and there is no formula I am aware of in which you have this—given the return current out of the cable and also the resistance, to find the resistance out of the end, which is the problem to be solved.

The discussion was then adjourned till the next meeting.

The following Candidates were balloted for and declared duly elected :—

FOREIGN MEMBERS :

H. Baron.
J. Ducoté.
A. Hequet.
J. Raymond.
B. Meyer.

MEMBER :—

George Stickland Criswick.

ASSOCIATES :—

Joseph H. Smith.
James Stott.
Ernest Edwards.
S. Sudworth.
J. M. O'Haire.
Lieut. H. S. Watkin, R.A.
C. E. Allen.
Henry Carlisle.
Benjamin Duff.
Cosmo Gordon Howard.
Major W. H. Collins, R.E.

STUDENT :—

Theodore Walrond.

The Meeting then adjourned.

The Forty-fifth Ordinary General Meeting was held on Wednesday, the 8th March, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The PRESIDENT: Before proceeding with the regular business of the evening, I regret to have to inform you that we have received from Mr. Scudamore a letter containing his resignation of membership. Mr. Scudamore, as you know, having left the country, and having retired from the Postal Telegraph service, thought it time to retire also from the Society. At the same time it affords me pleasure to have to announce to you the following resolution: "The Council of the Society of Telegraph Engineers, recognising the valuable services rendered to telegraphic science in this country by Mr. Frank Ives Scudamore, C.B., and past-President of this Society, accept, with great regret, his resignation of membership, and, in order to show the high appreciation by the Society of these his services, have appointed him from this date an Honorary Member of the Society." The Acting-Secretary has been instructed to communicate the resolution which you have so warmly received to Mr. Scudamore.

The PRESIDENT: A communication has been received from Mr. Willoughby Smith, on the Variation of the Resistance of Silenium, which the Acting Secretary will read before we resume the discussion on Dr. Werner Siemens's paper.

The communication was read accordingly, and will be found among the Original Communications at page 183.

The PRESIDENT; We will now, if you please, resume the discussion which was adjourned from the last meeting; but previous to doing so the Acting-Secretary will read a communication which has been received from Dr. Werner Siemens in reply to the remarks made upon his paper at our two last meetings. We shall then be happy to hear the further remarks which any one present may have to make. It seems to me that we have gone a little back

and out of the scope of the subject, too much into the early history of it; but there were two or three points in Dr. Siemens's paper which were worthy of comparison with our present knowledge on the subject. At the same time I think we have now almost exhausted its past history, and may, probably, have gone a little further into it than we intended; but that may have arisen from the questions started by the first two lines of Dr. Siemens's paper.

The Acting-Secretary then read the following communication from Dr. Werner Siemens:—

Berlin, 25th February, 1876.

If Professor Faraday, as Mr. Willoughby Smith stated, did not call public attention to the electrical qualities of gutta-percha until 1848, that certainly would prove the priority of our underground wires insulated with gutta-percha which were laid in the year 1847 between Berlin and Grossberen. In my paper, however, I had not intended to claim the first introduction of gutta-percha as an insulating material; what I claimed was the construction of the first machine for manufacturing gutta-percha covered *seamless* wires. Should anyone claim a right to priority in the construction or use of such gutta-percha covering machines, I should ask for statements indicating where and when such invention was published, because in questions of priority the first publication only is decisive. Whether gutta-percha bottles had been made before my invention, as was also stated in the discussion, is quite indifferent to the question of gutta-percha submarine wires.

Newall succeeded in laying a heavy deep-sea cable only by means of my theories of submerging cables, and by employing corresponding paying-out gear. Even if Mr. Brett had employed a steamer instead of a sailing vessel he would *not* have been successful unless he had employed a very powerful steamer with sufficient break-power upon the cable. Newall succeeded only after following my advice, which was to tow his sailing vessel by an Italian steam frigate, and to considerably increase the power of the breaks, cooling them with a stream of cold water. The forces brought into play in laying deep-sea cables were not sufficiently recognised at that time.

Mr. Varley appears to accuse me of attacking the theory of Longridge, that being a theory of very early date; but I should be much obliged to Mr. Varley if he would point out to me later theories in the submerging of cables.

With respect to Mr. Varley's arguments, I beg to say that I did *not* claim the first idea of, or the first laying of, submarine lines: Sömmering started the idea when proposing a galvanic telegraph. Underground lines were first laid on a large scale by Jacobi in St. Petersburg, using glass tubes and india-rubber wires.

Mr. Varley is correct in his remarks regarding the resistance which the rough hemp-covered cable surface offers. These remarks, however, do not alter my theories, because the friction of a sliding body in liquids always causes the nearest layer of the liquids to move, be the surface of that body smooth or rough.

I quite agree with Mr. Varley regarding the introduction of water-tanks on board of cable-ships, which I did not claim as my invention. I am not aware whether Jacobi made experiments in sea-water with his india-rubber covered wires. I used seamless gutta-percha wires in sea-water in April 1848, in the port of Kiel, fortifying that port by a system of torpedoes which were used during the war of that year.

I have nothing to say against Mr. Varley's remarks regarding the break-power required to allow the necessary amount of slack during the paying out of the 1865, 1866, and 1869 Atlantic cables, reducing the strain upon the cable to about one-third or one-fourth of the weight of the length of cable hanging perpendicular to the bottom. I also agree with Mr. Varley's assertion that, when hauling back the cable of 1869 from a depth of 2,400 fathoms, the resistance was so great that twice the strain due to the weight of the cable had to be applied. I am, however, fully convinced that a mathematician of such high standing as Mr. Varley will have no difficulty in finding, after a closer perusal of the formulæ and tables given in my paper, that his data fully agree with mine, when taking into consideration the velocities with which the cables were laid and hauled up.

Mr. Varley's claim of priority regarding the determination of faults in *looped cables* is so far correct, as Mr. Varley undoubtedly

found out that method independently of others; and he also was the first who published it in accessible journals. That very method, however, was already generally adopted in the years 1848 to 1853 by us for the determination of faults in insulated gutta-percha wire coils, as well as in double lines.

My formulæ for the determination of faults in single wires also include the possibility of finding the position of a fault by testing from one end only, the other end of the wire being insulated during one measurement, and to earth during a second measurement.

Mr. W. H. Preece: I read Dr. Werner Siemens's paper with great interest, and I have followed the discussion with equal interest. There is one point connected with the paper which struck me more than any other, and that is the very little regard that Dr. Werner Siemens has paid to his own personal interest, and to his own personal efforts, in furthering the advancement of submarine telegraphy. It is a misfortune, perhaps, that the discussion has, I might say, degenerated into questions of priority rather than into the examination of the principles brought forward in the paper; but that is what every new thing is more or less liable to, and submarine telegraphy is as yet but a little child. There are those in this room and living in England who have played a part in rearing this child, and who are naturally anxious when an opportunity occurs to blow their own trumpet. I must say, however, that Dr. Werner Siemens, who has had a grand opportunity for blowing his trumpet, has not taken advantage of it. There are very few persons who have done more to further telegraphy in every branch than Dr. Werner Siemens and his distinguished brothers. This paper is a valuable acquisition to the literature of telegraphy. It places the theory of the laying of submarine cables on a very plain and simple basis, and brings the various facts together in a way that can be easily comprehended by all.

We ought to draw a distinction between those who have exercised their ingenuity in suggesting pretty and novel ideas, and those who have actually assisted in driving the coach, that is to say, in rendering these ideas practical. Those who have pushed ahead the infant

child—telegraphy—undoubtedly deserve that what they have done should be placed upon the records of this Society. There is one individual who has made a great many telegraphic experiments, but because there was no telegraphic society in those days to report them they have almost dropped out of notice. In the Proceedings of the Asiatic Society for September 1839 there is recorded a series of careful experiments made by Dr. O'Shaughnessy (now Sir William Brooke), on a means of crossing rivers and carrying out submarine telegraphs, which were made early in that year, and the way in which those experiments were conducted is so interesting that with your permission I will read a short extract to you.

He says, "Insulation, according to my experiments, is best accomplished by inclosing the wire (previously pitched) in a split ratan and then paying the ratan round with tarred yarn, or the wire may, as in some experiments made by Colonel Pasley at Chatham, be surrounded by strands of tarred rope and this by pitched yarn. An insulated rope of this kind may be spread along a wet field, nay, even led through a river, and will still conduct without any appreciable loss the electrical signals above described."

At our last meeting we saw Sir Charles Wheatstone's interesting drawings of 1840, and heard a description of the cable that he proposed to lay between England and Belgium, but inasmuch as that suggestion was not brought to light till after a cable was actually laid it was not one of those steps which actually assisted in the progress of telegraphy. The first practical and actual experiment made across any water in Europe was undoubtedly made by our worthy President, Mr. C. V. Walker, in 1849. In 1850 the first actual submarine cable was laid between England and France through the exertions of Mr. Brett. The following year another cable was laid between Dover and Calais through the exertions of one whose name is not much mentioned—Mr. Crampton. Mr. Crampton was the man who found the capital and means and had the spirit and courage to submerge the first actual cable between England and France, and, more than that, he was the means of designing and adopting that form of cable which has never been departed from. The wire rope constituting the first cable characterises

every one laid since, and we can hardly be said to have departed in form from that early and first cable laid between England and the continent. True, there have been improvements in other directions—improvements in the core and in the strength of the external protection; we have had gutta-percha covered wire of various character, and we have also had those great improvements made by Mr. Hooper; but Mr. Crampton's wire rope remains the type of all submarine cables.

The paper deals with that very intricate point connected with the laying of cables, viz., the slipping back of a cable along an inclined plane when it falls from the stern of a ship to the bottom of the ocean. There are very few except those who have followed the subject mathematically who have succeeded in grasping the idea of this slipping back of the cable. There is no one who has put it in a simpler form than was done by Mr. Gravatt in this place many years ago, who illustrated it by a chain falling over an inclined plane. A cable falling at an angle through the water simply slips or slides through the water as a piece of paper slips or slides through the air (illustrated by a paper dart). To resist this tendency to slip down it was only necessary to apply to the cable a break-power equal in weight to the length of the cable which would be suspended from the stern of the ship to the bottom of the sea.

Now, in all these papers read at different times, unfortunately we have a great deal of theory and not much practice. There are many gentlemen in this room who have been occupied in laying thousands of miles of cable in deep sea; but we have never had any practical details of the break-power used, the variation of break-power according to the depth of water, the angle which the rope makes with the stern of the ship or with the horizon, or any facts which tend to prove the truth of the theories brought forward. I must say I was in hopes, after the experience of the Messrs. Siemens in the Atlantic and Mediterranean, after the experience of Mr. Webb and of others in other parts of the world, that we should have had some practical proofs of the truth of these theoretical problems.

There is a discrepancy between the formulæ of Dr. Werner

Siemens and those which were established in this building some years ago by Messrs. Longridge and Brooks, whose formulæ I have every reason to believe very nearly accords with practice. They showed that the friction of the rope and water follows in terms of the square of the velocity, whilst Dr. Werner Siemens states it is not as the square but as the velocity itself. Without some reasons given, and without some practical results proving the formulæ he has produced, I should be chary in accepting Dr. Werner Siemens's theory that the friction of a falling cable varies simply as the velocity. We know when water or air flows through pipes that the friction varies directly as the square of the velocity; and what difference there is between water flowing within the internal circumference of a pipe or the outside circumference of a rod falling through water I do not myself see. If the friction of water varies as the square of the velocity in a pipe then the friction of a rod through water must also vary as the square of the velocity. So that without some reasons or facts to support it I should not like to accept Dr. Werner Siemens's statement that the friction varies as the velocity only. At the same time I am quite sure so high an authority as Dr. Werner Siemens would not make such a statement without good and proper reasons for it. So that I hope we may yet have his facts for making that statement.

With regard to testing, to which the second part of the paper refers, if our thanks are due to anybody for having placed the subject of testing on such a basis as it is now, we are indebted very highly to the Messrs. Siemens. It is true we were able to ascertain with considerable exactness the locality of faults. It is true the ordinary theory of testing was thoroughly well known to Mr. Varley, myself, and others who have been engaged in repairing cables; but that beautiful apparatus—the form of bridge which is now so universally used, by which we are able to vary the ratio *between the two branches of the bridge*, is due to Messrs. Siemens, and was one of the first steps taken to place the modes of testing on a footing of such great exactitude. The way in which Dr. Werner Siemens refers to the mode of eliminating earth currents and currents of polarisation received very full

elucidation before this Society on a late occasion, when Mr. Fahie gave a paper showing how to eliminate those disturbing causes, and enabling us to arrive at the exact distance when a wire is broken. Though Dr. Werner Siemens has treated this subject with his usual thoroughness, I do not see that he has advanced the subject much further than it was placed by Mr. Fahie.

Dr. Werner Siemens has not alluded to a subject which affects us very much—that is, the durability of submarine cables. It is very well to lay and work cables, and tell us how far off they are broken; but all our knowledge is of little use unless we are able by some means to secure durability in the structure of the cable. It is one thing to lay a cable, it is another to bring it to the surface again. We have advanced very little from the first cable of 1851. We have altered the materials in the structure, and particularly those of deep sea cables, but I do not think we have yet reached perfection. The last Atlantic cable laid is a sample of all those previously laid, and this form of deep sea cable is radically bad. It does not give us a cable of such a form that we can at a future time be certain that we can bring it to the surface again. In the Atlantic cable we have an outside covering of iron wire and hemp, which in course of time must rust and decay.

We have hopes that amongst those engaged in the maintenance and construction of cables some telegraph engineer may be found to invent a cable which will enable us to lay it with the certainty of durability equal to that of gutta-percha itself. We know practically gutta-percha to be indestructible. There is no proof of the decay or deterioration of gutta-percha in sea-water. We shall not consider ourselves in the possession of a perfect deep sea cable till we have one so protected by durable materials that we can at any time raise it to the surface if it should be required. I must, however, qualify the remark that gutta-percha is indestructible in sea-water. I forgot for the moment that the teredo had tasted its sweets. We have seen specimens in this room of the ravages of marine animalculæ and crustacea upon gutta-percha; therefore, I withdraw the statement that it is indestructible, and will simply say that, till we succeed in getting an outside coating for cables as strong as the present form and as durable as gutta-percha, we shall not reach perfection in the form of our deep-sea cables.

Mr. J. A. Longridge, C.E.: I have been kindly invited to attend the meeting this evening; and, with your permission, I will make a few remarks upon the paper which is under discussion. I was not present at the reading of the paper, and I have only had the opportunity of seeing it since last evening, when I received a copy of it; consequently I have not been able to read it carefully through. I will, however, make two or three remarks upon it, which, I think, I am entitled to do, the more so as Dr. Werner Siemens has referred to a paper written by my friend Mr. Brooks and myself, and read in this room eighteen years ago. It will be in your memory, Sir, and probably that of other members, that at that time there were great doubts whether a cable could be laid along the sea bottom in a straight line. There had been a discussion by the British Association at Dublin, in 1857, in which the conclusion was arrived at that without infinite velocity of the ship such a thing was impossible. I will not go back to those old discussions, but simply refer now to what Dr. Werner Siemens says with regard to my paper.

He says, "The mathematical part of the treatise is not to be disputed, and gives an accurate description of the curve formed by a cable suspended in an oblique direction in water, if paid out with a strain upon the sea bed." Now, Sir, that is the problem under its most general form. It is a very easy problem indeed to solve what would be the position of the cable if there were no strain at the bottom. The other is a much more complicated problem, and Mr. Brooks and myself felt that unless we dealt with the general form we should be only investigating an empirical method (which is not the course which scientific men generally like to take). The investigation must be to show the form of a cable with a strain on the bottom. That included the case where there was no strain, simply by equalising the bottom tension to zero; and there we showed distinctly the cable would take a straight line. But Dr. Siemens goes on to say: "The physical part of the work, and the practical consequences drawn from it, are open to grave objections." I scarcely understand what he means by "the physical part of the work." As to "the practical consequences drawn from it," I am prepared to state without

fear of contradiction that all subsequent practice—at least all successful practice—has been founded upon principles shown in that paper to be mathematically the correct principles. We dealt then with the various questions which arise in the laying of a line of cable and we have dealt with them seriatim. But Dr. Siemens goes on to say, "One of the first principles taken for granted which materially influences the results is incorrect." That is a grave accusation for a gentleman in Dr. Siemens's position to make. I agree with the last speaker that it must be assumed he has good and substantial reasons for making such an accusation. He refers to the assumption which he says we made that the friction of a cable sliding down through water was proportional to the square of the velocity, whereas he asserts it really is proportional to the velocity itself. Now a statement like that coming from Dr. Siemens, a man of the highest possible reputation, must have some basis, and it is to be regretted that he has not pointed out wherein that basis consists. I will tell you where we get our data from, viz. Colonel Beaufoy's experiments. He was the only man I know of who made a thorough series of experiments on the subject of the lateral friction of a body passing through water. I have referred to them and I find it varies from 1·8 to 1·947: that is practically nearly as the square of the velocity. But it does not altogether rest upon that. This subject has been discussed in the Institution of Civil Engineers more than once. There was a paper read in 1857 by Mr. Armstrong on "High Speed Navigation," in which, of course, the element of surface friction was considered; and a gentleman for whose opinion I have a very high respect, and I think that is the case with all in this room, Mr. Hawksley, past President of the Institution of Civil Engineers, in that discussion stated distinctly that the resistance of bodies passing through water varies exactly as the square of the velocity, and no person in that discussion, and it was a very long one, in any way contradicted Mr. Hawksley's statement.

Again, in 1866, Mr. G. H. Phipps, member of the Institution of Civil Engineers, read a paper on "The Resistance of Bodies passing through Water," in which he referred to Beaufoy's experiments, showing that the resistance was proportional to the 1·949 power ot

the velocity, and all who took part in the discussion—Mr. Bidder, Sir John Hawkshaw, and others—coincided with that view, and I never heard a doubt expressed as to the fact of the resistance being as the square of the velocity. We know it is so in pipes; and to say that there is one law for the inside and another law for the outside of a pipe is a thing which does not commend itself to my mind; consequently I think Dr. Werner Siemens ought not to contradict, in the positive way he does, a paper which has been pretty well discussed, without stating some grounds on which he disputes its correctness; but he goes on, and brings out a formula of his own, and works out results from it.

On page 9 of the paper it is stated: "The difference between these formulæ and ours arises from the supposition that the force of sliding friction obeys the law of squares." If we are wrong our formulæ are also wrong. But there is not an item in Dr. Siemens's formulæ which is not ours, excepting the hypothesis; and I assert that our hypothesis is the correct one and not Dr. Siemens', and that our formulæ are correct and his are incorrect. I think, Sir, Dr. Werner Siemens is scarcely justified in claiming that the cables, which have been laid, have been laid upon *his theories*, as he has done in the letter read this evening; whereas the theory which was propounded eighteen years ago has never yet been controverted that I am aware of, and certainly has not been so by anything which Dr. Siemens has advanced in the paper under consideration.

Then Dr. Siemens further states: "The work is deficient as regards a clear perception of the principal factors and a lucid exposition of the results." Perhaps they are not very lucid; a question of high mathematics is rather difficult to make lucid to the general reader. As for saying there is no clear perception of the principal factors, *i.e.*, the principal conditions of the problem, I believe it will be seen that we have dealt with a great deal more than Dr. Siemens himself has dealt with. At the end of the paper we have supplied an Appendix, in which there are some ten or twelve problems stated. The first of these is: A body descending vertically in a resisting medium. That we solved. The second one was: A body descending obliquely, such as a continuous cable,

under the same conditions. That also we solved, and added two or three corrollaries showing: (A) The velocity at which the cable runs out vertically without tension; (B) The angle at which the cable would run out with the greatest velocity; (C) The waste of cable when it runs out at any given angle free from tension; (D) The angle of motion of the end in case of fracture. The third problem was the general equation to the curve, from which we deduced that the cable descends in a straight line when the bottom tension is zero. The fourth problem was the equation for tension—what Dr. Siemens calls the brake-power. We showed in two tables the reduction of tension caused by running out certain amounts of slack, applicable both to a heavy cable and to a light one. The fifth problem showed the waste of cable in passing through currents. The sixth was the equation to the curve of a line strained across a current. The seventh was the tension due to the friction of water on a current coming across the line of a cable. The eighth was the form which the cable would assume and the resulting tension supposing the paying-out apparatus was suddenly stopped in the act of paying out. The ninth problem was to find the portion of the end of a cable at any given interval of time after fracture. The tenth was the extension of length due to the compression of the inner core. The last problem was the investigation of the variation of tension due to pitching and the movement of the ship in a heavy sea, and the motion of the paying-out apparatus. We further investigated the effects of floats on resisters, and the means of saving cable in case of fracture. These were the principal problems we solved. We solved them with mathematical correctness; and, from all I have seen since, I believe the results we arrived at were practical and reliable results, and not, as some say, theoretical results. Therefore, I say again, Dr. Werner Siemens, in saying there was not a clear perception of the principal factors, has gone further than he ought fairly to have gone, and that we have treated the subject far more in extenso than he himself has done, and with, I believe, greater accuracy. So far as I have seen, the only difference he has made is assuming the friction to be simply as the velocity and not as the square, and that, I believe, is an error. With regard to Mr. Gravatt, he simply showed the

question of a chain lying over an inclined plane with part of the chain down the incline and the other hanging over the end vertically. That was a very elementary problem of mechanical science—that the two will balance; but to say that it solved the question with regard to telegraph cables is going a little too far.

There is one point I would draw attention to, viz., the conclusion which we arrived at as to the absolute necessity of laying light cables. What I ventured to state eighteen years ago has come true; and the evil of a heavy cable has been attempted to be remedied in a way I do not approve of, viz., by making a heavy cable, and then making it light by putting on hemp. Ten years ago I predicted that if a heavy cable, after being laid a year or two, happened to break in 2,000 fathoms of water, you would not be able to get it up to the surface. I still believe so. I think one of the Atlantic cables, two or three years ago, happened to take that freak in 2,000 fathoms. I think it was the 1865 cable; but, as far as I know, that has never been brought up. They have tried to bring it up more than once, but failed, and I believe they will never succeed, for this reason: the iron, being in a state of oxidation, is even heavier than before, and it has lost its strength; the hemp is gone; there is no strength in the thing; and when you try to lift it it breaks. If the cable is required to be lifted immediately after it is laid, but possibly before the iron corrodes, you can do it. A heavy cable is not only more expensive in first cost, but it takes a large ship to lay it. I maintain that about a five-eighths diameter cable might be made with the same conducting power, and could be laid more safely, and could be got up with greater ease. I should like to see cables payed out as log lines from a ship. The proper apparatus is one which pays out the cable with the proportion of slack you want, not one which holds it in. Some years ago I had the model of a machine, which I showed to the Joint Committee on Telegraphs, as well as some drawings of another system, which was made to imitate the action of a man's hands. It was so arranged that the cable passed through two long belts with a certain amount of friction. If the ship pitched heavily it slid through the belts; in case of a kink the machine opened and allowed the kink to pass, and the cable was then replaced in the apparatus.

By this means the cable was drawn out of the hold and payed overboard. With a light cable there would be comparative ease in getting it up; but I do not think you would require to get it up. The reason why the 1865 cable broke, I have no doubt, was its being laid over a kind of valley, and it hung across this valley like the chain of a suspension bridge. As long as the wire held good it was all right; but when that rusted the catenary strain broke the cable, and down it went. Feeling this, as I have done for many years, I have always believed that the day will come when we shall abandon all this heavy wire round the cable; that it will consist only of the copper wire and perhaps fine steel wires bound inside the gutta-percha and protected by some material sufficient to protect it until it has got safe to the bottom. Supposing such a cable is laid with a due amount of slack—not hanging over precipices—then I believe when you get a cable like that laid in 2,000 fathoms, you will never require to touch that cable again.

Mr. WM. HOOPER: The last observation of the gentleman who has just addressed us is entitled to consideration—that is as to the durability of cables. There is no difficulty in submerging them if you only get the right sort. I do not pay much regard to the question whether you have apparatus or not. You must lay such a cable as can be raised for a fault, and where the specific gravity is not so much thought of as the external covering of the cable. If you have iron wire outside the core the wrought iron loses strength and after a time it becomes a burden more than an assistance. It is not required in sinking a cable to have ironwork at all. The decay of iron wire under water has been established, and therefore the great point is to get an indestructible insulator, and whoever is clever enough to find out a good covering to a good insulator will accomplish what has not yet been attained. My name has been mentioned in connection with this subject. I should have been glad if we had carried out one element in cables which Major Champain brought before us a few weeks ago, viz., an insulator which will last. Marine insects do bore into gutta-percha, and I should have been glad if Mr. Preece had communicated the fact that a form of insulator has been discovered which is not injured

by these boring insects. The teredo does bore into gutta-percha, but I have not heard that india-rubber has been attacked. I am not aware to what extent the Atlantic Cables in deep water are affected by this cause; but in the case of the Persian Gulf and Red Sea cables it is found that they are bored into by insects through the gutta-percha, and the cables have failed more than once from that cause.

Mr. WEBB: I was rather cut short in my remarks at the last meeting for want of time. I was then speaking about dynamometers, and I described the form used in the first Altantic cable in 1857, and that which was used in 1858, and which is used by the Telegraph Construction Company and other firms, and is now generally well known. I also described the first dynamometer used by Mr. Newall, and suggested by Mr. Werner Siemens, in laying the cable between Bona and Cagliari, and after that the one used in the Red Sea line.

The dynamometer I am now going to describe is one I designed in 1867, and it was fitted in the "Narva," and also in the "International." It is shown in figures 1, 2, and 3. A is a V-sheave on the end of a shaft carried at the extremity of braced framework which has an axle at B on which it can turn.

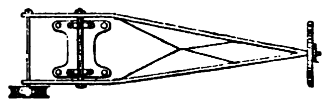


Fig. 1.

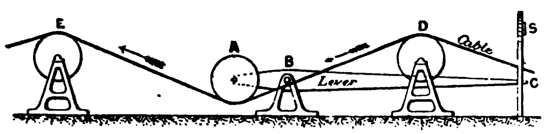


Fig. 2.

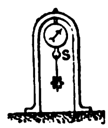


Fig. 3.

The distance B C is five times A B. The end C of this framework is attached by links to a Salter's Balance S suspended from an arched frame.

The cable passes over two sheaves D and E, and under the sheave A. These sheaves are placed so that the cable forms with the horizontal such an angle that its sine is 0·25, or in fact 14° 31′. It

will be seen by resolving the forces that a strain on the cable of 10 tons will only give an upward strain on the sheave A of 5 tons, the lever A B C having a leverage of 5 to 1, the strain at C on the Salter's Balance is only one-fifth of this, or 1 ton, so that every pound shown on the Salter's Balance indicates 10 lbs. strain on the cable. The instrument has its advantages; it is much lighter than the ordinary dynamometer, and lies much lower. There is no heavy weight dancing up and down, and there is no changing of weights and scales required, many hundred miles of cable having been laid with this dynamometer. The French Government have adopted it in repairing ships.

With regard to the question of light cables, Mr. Longridge, who I have before had occasion to meet, and for whose paper I do not suppose anyone has a greater respect than I have myself, has spoken as an engineer about our employing light cables, and Mr. Hooper has spoken of light cables; but all who are engaged in submarine telegraphy know that this is one of the problems yet to be solved. When people say, "Oh! lay down a mere copper wire insulated with gutta-percha," they do not consider that if you have such a cable as that it will break. I have made experiments with regard to the breaking-strain of several kinds of copper wire insulated with gutta-percha, and in every case it came out equal to about the weight of two miles of the wire and gutta-percha in water, so that if you stopped the ship in two miles depth of water the cable would simply break; the question is, what you are to put round it to make it so that you can lay it, in the first place; and then you must allow this—that whoever has to lay a cable, or whoever makes it, you may have faults, which occur from various causes, and therefore you must be prepared to stop the ship and haul back the cable, and this may have to be done in heavy weather; therefore gutta-percha covered copper wire alone will not suffice. Then you come to the question of hemp placed over the gutta-percha. That is a question which is open to discussion, and there are cases in which it has been found that hemp increases the liability of the line to kink, and thus makes it difficult to coil. These are all questions which have yet to be solved. It is all very well to talk about light cables, but let some one bring us a pattern of cable that

can be taken from the factory, coiled and wetted, and laid to dry, taken on board ship and laid down, and, if necessary, hauled back without breaking. When anyone brings such a specimen of cable forward, let it be discussed. But up to this time, as far as my experience goes, I have seen nothing which satisfies these conditions.

Mr. Treuenfeld: Mr. Varley, after describing briefly the external construction of an Atlantic cable, says, "Now the whole of this presents a smooth exterior, but the moment the cable is passed round the drum it becomes flat in shape and you have a number of fibres sticking out of the cable. We now find the cable bristling all over with filaments." I do not know to which Atlantic cable Mr. Varley refers, but the cable that becomes flat on the drum must be inherently defective. Neither do I follow Mr. Varley in his drawing of what may be termed a "porcupine cable;" there may be fibres visible upon close examination, but they certainly are not of the exaggerated proportions that I understand Mr. Varley to give them.

We all know that well-manufactured cables have been raised out of the deep sea after having months' immersion and did not show any signs of flatness or filaments, but looked as perfect as when in the manufacturers' tanks.

I perfectly agree with the remarks made this evening by our worthy President, that the lengthy arguments on priority which followed the reading of the papers have been rather out of place, but I think the fault lies on the side of those who raised the question and not those who answered them. I have several remarks which I intended to bring against the lengthy historical arguments of former speakers, but following the President's desire I will only sum up the conclusion to which I arrive and add a few words which ought not to be forgotten in this discussion, viz. that at the period when the Russian Government was manufacturing and laying underground wires, the necessity for methods of measurements, resistance standards, and methods for the determination of faults, arose and was overcome by Dr. Werner Siemens, who used such methods and established the rules for testing as well as creating the unit of resistance, and handed over these methods and units to the electricians of English manufacturers of submarine cables.

The first methods for testing wires electrically and determining faults were published by Dr. Werner Siemens at the meeting of the great "Academie des Sciences," 29th April, 1850, including the laws of electric charge and discharge in cables. These investigations were also printed in the archive of the great Academie, also in "Berlin Physicalischen Gesellschaft," 18th January, 1850, and in "Poggendorf's Annalen," vol. 75, 1850. In the latter publication (at page 497) lightning-protectors with plates or points were mentioned for the first time, and the insulation of a line for the first time given in units of resistance. Also the first formulæ and methods for the determination of faults are given on pages 493 and 494; finally the laws of electrical charges in underground wires page 499.

I agree with Mr. Willoughby Smith that it is a "curious" fact the great philosopher Professor Faraday brought before the public *as a new thing*, investigations relating to gutta-percha, which Dr. Werner Siemens had previously published. Faraday *re*-discovered Siemens's laws of charges, but he afterwards acknowledged Dr. Siemens's priority with regard to the same.

If we now consider that the laying down of determined methods and formulæ for the expression of electrical resistance and the finding of faults not only started from the pen of Dr. Werner Siemens, but that these very methods, which were necessarily brought into life during the manufacture and laying of the thousands of miles of gutta-percha underground wires in Prussia, afterwards formed the base for submarine testing, and if we consider that those Prussian underground wires were made with a machine for the same principle as those of our days, and that gutta-percha was recommended and adopted as an insulating material for cables, then I think every impartial judge must come to the conclusion that, "*The starting point of submarine telegraphy is to be found in the subterranean lines constructed in Prussia during the years* 1847—1852."

The PRESIDENT: This now brings to a close a most valuable paper and interesting discussion. I have no further remarks to offer on the subject, nor even if I had would time let me, for our

hour of closing is already passed. I have therefore only to ask you to accord to Dr. Siemens a hearty vote of thanks for his communication.

The vote of thanks was carried with acclamation.

The following Candidates were balloted for and declared duly elected :—

FOREIGN MEMBERS :—

M. J. F. Niermeyer.
M. L. W. Courtenay.

MEMBERS :—

Viscount Bury, K.C.M.G.
Lieut. Skinner, R.E.

ASSOCIATES :—

Mr. H. Marsh.
,, H. D. Wilkinson.
,, J. Crawley.

The Meeting then adjourned.

[APPENDIX.]

NOTES on DR. WERNER SIEMENS'S PAPER "On SUBMERGING and TESTING SUBMARINE TELEGRAPHS."

These notes are obliged to appear in their present form, as I was unable to attend the adjourned meetings on the discussion of Dr. Siemens's paper.

There are three distinct kinds of faults which may happen to the cable:—

1st. Rupture of the conductor with the ends of the copper inclosed in the insulating envelope, so as to give partial or complete insulation on one or both sides of the break.

2nd. Rupture of the conductor with the ends inclosed in the insulator, and giving partial or intermittent contact.

3rd. Rupture of the conductor with one or both ends coming in contact with the water.

The measurements for the position of the rupture by means of discharge tests are well known, and on shore can be easily carried out; but on board ship and with marine galvanometers it is not so easy nor so certain to arrive at the exact position. At sea I have found the best plan to be, when using the capacity test, to read with as small a swing of the mirror as possible; and, when comparing the discharge from the cable with that from a condenser, to vary the shunt resistance so as to obtain as near as possible the same range of swing.

The method pointed out by Dr. Siemens in measuring a series of abstracted charges from the cable is evidently of no value if the cable gives much loss at the break, or if the cable itself leaks fast. Apart from these objections, the accumulated possible errors in the result would, I should imagine, be of some importance.

In dealing with a case where the loss is not very great, or at least so small as not to be perceptible during the time of taking an observation, I know of no better plan than using a portable electrometer, the indications of which can be read quite independently of the motion of the ship, or the length of cable under test. The plan is first to approximate by

discharge the position of the break; then connect up a series of condensers, or some lengths of spare cable approximately equal to the indicated capacity, with a portable electrometer and battery. The reading being taken, the battery is removed, the charged spare cable or condensers being left connected to the electrometer. The end of the cable to be tested, previously discharged, is now applied to the electrometer and condensers, and the reading again carefully noted, from which the position of the break is most easily and reliably fixed upon.

It is the simplest and most certain process to obtain the measurement by halving, if possible. Thus, supposing with the condensers and electrometer the reading was eighty turns of the micrometer screw, and when the cable was connected up it fell to forty, the capacity of the condensers will be exactly the capacity of the cable to the break, the capacity of the electrometer being so small as to be neglected.

The case of partial or intermittent contact is an extremely unpleasant one, and requires very great care. With vulcanized rubber cores the simplest plan is to have a current flowing through the cable. After a time the ends become coated with a comparatively insulating film of sulphide, when an induction test can be applied.

When the two ends can be got at, and the resistance at the break is not high enough to depend upon the result of a simple discharge test, the better plan is, first, to charge the cable at both ends, and note the discharge, then take the discharge from each end separately. Now, as we are likely to charge up both portions of the cable, the two discharges added will be greater than when the two ends were tested together. Suppose, for example, with the two ends together our discharge equals 100, and with one end only we get 70 and on the other 40, we have to divide 100 into two parts, having the ratio of these numbers for the capacity on either side to the break. However carefully the test be made the actual position of the fault will always be nearer to the end on the shorter side. The higher the resistance at the break the more nearly will the actual position of the break coincide with that indicated by testing. Probably by measuring charge instead of discharge better results may be obtained.

In the third case, when the ends are exposed to the water, it has been proposed to coat the ends with a sub-chloride of copper by means of the current, and to measure the discharge. I have not yet met with any method for dealing with this kind of break which can be considered satisfactory, and more especially if the operations are carried out only at

one end. In localising such a fault, and in fact in dealing with all faults generally, no precise line of action can be marked out; more must depend upon tact and perseverance than on any prescribed regulation as to testing.

The earlier tests taken on a broken cable are always more likely to be useful for pronouncing on its position than those taken after the lapse of a longer time, partly from the corrosion of the copper, or the conductor being taken away for a short distance inside the insulator, which will increase the resistance enormously. If a cable has taken a few days to break asunder, as happens under the action of a chafing or abraiding motion, the tests, if the cable has previously to breaking showed indications of being faulty, will be of great assistance. In this case systematic testing from both ends should be immediately commenced; and such time for testing should be selected when the cable is least disturbed by earth currents.

By these precautions a fault was localised in the cable between Para and Cayenne, which ultimately resulted in a break. The day on which interruption first showed itself was occupied in testing and with satisfactory tests as to the position of the fault; but, although the cable was tested several weeks afterwards, not a single test could be obtained from either end which was of the slightest value: so that, although this fault was not removed until February, I elected to act upon the tests taken in October for the place of operation. Whilst signals could be transmitted each station exchanged their results every day, so that the repairing steamer calling at one station had the data for operating without the necessity of going to the other end of the line.

In measuring the resistances for such a fault, as well as for extensive insulation faults, it is best to bring the fault by means of the battery to as constant a condition as possible. The power required for this must depend upon circumstances; it is as difficult to work upon a very open fault as it is on a small and varying fault. Having fixed upon the number of cells best suited, the resistance is carefully measured, which by reversed currents and "earthing" alternately can frequently be made constant. If the cable is much disturbed by earth currents or the chemical action at the fault, it is better to measure the resistance by noting the deflections on a dead-beat galvanometer, recording the deflections at frequent and short intervals whilst the battery is on, and then for the same and similar intervals of time taking the deflections on the galvanometer when the cable is to earth, adding or subtracting the sum

of these readings according as they are on the same or opposite side of zero, as the first set of readings. The mean of the algebraical sum of these deflections is then reproduced by resistances, using the same battery-power and connections as with the cable. In this way an adjustment is carried out which eliminates all the variations to which the fault might give rise. Although I believe this method is well known to most experienced electricians, I mention it here as it is not so well known as it deserves to be by electricians generally.

Before concluding I may add that I have seen and also devised several methods for dealing with small faults; but at present I feel no hesitation in saying that a good method for localising small faults in submarine cables remains to be enunciated.

THOMAS T. P. BRUCE WARREN.

The Forty-sixth Ordinary General Meeting was held on Wednesday, the 22nd March, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The PRESIDENT: The discussion on the paper "On Submerging and Testing Submarine Telegraphs" by Dr. Werner Siemens was necessarily closed at the last meeting without the usual reply and remarks of the author of the paper. Dr. Siemens is not in this country, but he has seen the notes of the discussion and has sent a reply thereto in addition to the communication already read (p. 100.) This will now be read before we enter upon the other business of the meeting.

The ACTING-SECRETARY then read the following communication from Dr. Werner Siemens:—

"Mr. J. A. Longridge has at the meeting of the Society upon the 8th of this month (March) subjected my paper to severe criticism. Mr. Longridge says that I have only adopted his work of eighteen years ago, and suggests very plainly that my ideas and formulæ are mere transpositions from his own, under simpler conditions arising from an erroneous introduction of the sliding friction of water. In answer to these severe reproaches I have to enter into the matter more particularly.

The treatise of Messrs. Longridge and Brooks is, so far as I know, exclusively published in *The Proceedings of the Institute of Civil Engineers*, vol. xvii. for 1858, consequently one year later than the demonstration and employment of the theory by me on the first successful laying of a deep sea cable, that of the Bona-Cagliari cable. This theory was founded on the supposition of the falling cable taking a straight line, and the sliding friction in the direction of the cable-laying being simply proportional to the sliding velocity. Mr. Longridge is therefore scarcely entitled to assume that the first idea has been adapted from his treatise published one year later.

It is to be regretted that, so far as my knowledge goes, Messrs. Longridge and Brooks' paper has not appeared in a generally accessible scientific journal, and this explains how it is that I derived, only in the summer of 1874, on board the "Faraday," from our engineer Mr. Brittle, the knowledge of the existence of this treatise at a time when my ideas had long since been formed. As in the scientific world it is customary that the right of priority is accorded to the first publication, I have, without consideration of my independence, acknowledged the right of the priority of Messrs. Longridge and Brooks in the purely mathematical treatment of the subject, and I have confined my own claims to the rectification and further development of their formulæ.

Mr. Longridge should be thankful that I have withdrawn his treatise from its eighteen years' concealment.

That my formulæ and conclusions are not contained in his, follows as a sequence from the fact that I am declared to have wrongly stated one of the principal factors of the calculation—the law of the sliding-friction in water—which Mr. Longridge has accepted and for which I have substituted another. Moreover, telegraph engineers experienced in cable laying will be best able to judge whether these formulæ and tables are the same, and are not more readily practicable than those given by Messrs. Longridge and Brooks. Notwithstanding this, my difference from Mr. Longridge concentres itself in the question upon the law of sliding friction in water.

Mr. Longridge finds the expression used in the English translation of my paper, "the physical part of the work," incomprehensible. I willingly concede him to be right in this, as it is a too literal translation of the German text. In Germany by the term *physik* is understood what is known in England as *Natural Philosophy*. The intention was to express that his treatment was so far mathematically correct, but that the foundations of the calculations and therefore the results of the calculations were incorrect.

Mr. Longridge contests this, and seeks to prove the correctness of his ideas from given authorities, and blames me for the facility with which I declare error, without proving by experiments a law commonly accepted as correct, to be incorrect.

He also finds it absurd to accept a different proportion of the friction at the inside and at the outside of a tube.

I very well know that practice has established empirical formulæ for the friction of liquids in long tubes in which the velocity appears at the same time in both the square and in the first power. Similar empirical formulæ are also established for the motion of water in channels and open gutters, and these are undoubtedly correct to the extent of the experiments on which they are based.

Eytelwein has theoretically based the quadratic law for the flow through tubes on the view that with double the velocity twice as much adhering water must be torn away with twice the rapidity from the sides of the tube. He has not considered that the water on the sides of the tube does not flow at all, but remains without motion, and that the velocity increases gradually towards the centre, so that a concentric stratum of water thrusts itself forward on the adjacent strata with less velocity. In tubes of smaller diameter and with not too great water-velocity we obtain, as has been often ascertained, an outflow of water which is directly proportional to the pressure difference. I myself have shown this to be undoubtedly the case by a lengthy series of experiments published in *The German Telegraph Journal*, vol. xiii. of the year 1866. These experiments were made for the purpose of showing the practicability of such long pneumatic tubes as were intended for use in Berlin, and which were carried out. That with wide tubes and high-current velocity there appears a quadratic-velocity factor, besides that of the first degree, is accounted for by the liquid vortices arising in the tubes by which *vis vivâ* is consumed. The same thing occurs with the motion of water in channels and with other practical experiments from which surface-friction has been concluded.

But I am obliged to compare the authorities quoted by Mr. Longridge with others whose competence he will scarcely doubt:—

Sliding Friction in Liquids.

Newton (Principia Mathematica Philosophiæ, lib. ii. sec. 9, 1687) says that the friction exerted by two strata of fluids moving in the same direction is proportional to the difference of their velocities, and in proportion to the contact-surfaces of the two strata.

a. Oscillation of a Plate in a Fluid.

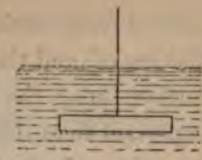


Fig. 1.

Coulomb (Mem. de l'Inst. National, tome iii. p. 261) finds that the resistance between solid bodies and liquids is proportional to the velocity; if there is a change of liquid there are two terms, one proportional to v and the other to v^2.

O. E. Myer (Pogg. 113, p. 55 *et seq.*) corrects the theory under the same supposition with reference to velocity and finds agreement with observation; measurements with a series of liquids, plate-glass, brass, and tin-plate.

b. Oscillation of a Liquid-filled Hollow Sphere.

Fig. 2.

Helmholtz and Piotrowsky (Sitzung berichte der Wiener Academie, 1860, Bd. xl.), Theoretical Treatises—under the supposition of proportionality to the velocity—agreement with observation —experimental determination in a series of liquids.

c. Mötion in Tubes.

In narrow tubes *Poisonelle's* law is exact; this has been experimentally found by Poisonelle and has been theoretically determined by several authors, under the supposition of proportionality to velocity.

Fig. 3.

For the sliding cable the same law must exist, because here (fig. 3) as in the capillary tube the length is very great in proportion to the diameter: it has to be compared directly with the motion inside a tube; the tube in the case of the cable is that stratum of water in which there is no motion; instead of the column of water in the tube the cable has to be imagined, the whole difference being in the coefficient for friction of the central liquid cylinder which differs from that for the cable.

Hagenbach (*vide* p. 404) shows that Poisonelle's formulæ are adaptable for all tubes whose length is very large in proportion to their diameter.

In wide tubes the law is complex, $av + bv^2$; all observers agree that the dependence is greater in proportion to v and smaller in proportion to v^2. Hagenbach shows that a complex law $av + bv^2$ agrees with observation, and states that the quadratic term is

derived from vortices which at greater velocities occur through the shaking of, and the irregularities in, the tube. The effect of these vortices upon the central flow in the tube must be much more important than that of the vortices upon the cable which may arise from the cable itself. Then, also, in the case last mentioned the vortices can escape on all sides; in the first case they meet the resistance of the tube.

There is, however, no ground for the acceptation of vortices, as the case of the sliding cable is wholly comparable with motion in the capillary tube. And this motion is in accordance with the acceptation of the case of cylinders sliding one upon another.

Mr. Longridge will understand from this that I have not, as he thinks, frivolously overthrown a commonly accepted principle and substituted one upon other motives. He has committed the common error of enlarging to too great an extent upon the basis of experiment. But I had in the year 1857, when I formed my theory of cable laying, convinced myself by direct experiment that with the motion of a rope in a straight line in water the velocity produces a directly proportional resistance. I had, through a perfectly calm sea, a log-line dragging astern of the cable-ship, the line being fastened to a spring-balance. The velocity of the ship was measured by means of an ordinary ship's log; the resistance measured in kilogrammes agreed exactly for different velocities of the vessel with the speed shown by the log. Mr. Longridge can, by this experiment, easily convince himself of the correctness of my assumption and of the incorrectness of his formulæ. But I advise him to make this experiment in calm water only, because every wave-motion gives incorrect results. I hope Mr. Longridge will comprehend that he has gone too far in the defence of his formulæ, and will acknowledge the injury he has done me."

The President: I have now to call upon Mr. Langdon to read his paper which has been announced for this evening upon a subject which, at the present time, is practically interesting, viz. on "Electric Repeaters for Railway Signals."

ELECTRIC REPEATERS FOR RAILWAY SIGNALS.

By Mr. W. Langdon,
Superintendent, Post Office Telegraphs.

Attention has recently been directed, in a very forcible manner, to the employment of electricity for repeating back to signalmen, and others, the condition of the signals worked by them for the control of traffic on railways. It would appear that one of two courses is needed: either that signals themselves shall be so constructed as to be removed from the influence of the elements, or that some means shall be adopted by which the condition of the signal—the arm, or disc, by day, and the light by night—may be brought clearly before the man who is answerable for its due operation.

To alter the form of signal in use upon any line of railway would not only be a question of considerable expense but also one of some difficulty. Moreover, it is doubtful if a system could be found which would prove independent of those frequent and often violent atmospheric changes of which we have of late had such ample experience. The present system of working signals is undoubtedly that which would recommend itself to every practical mind as the best for general use. At the same time it is equally evident that it must be subject to every change of temperature. During a hot day the wire by which the signals are worked expands, and under the colder atmosphere of night it again contracts. A change in the wind from a warm to a cold quarter will have the same effect. Some two months since the author watched the working, or rather attempted working, of a distant signal for over an hour, during which, although the lever in communication with it was operated in the usual manner, the arm never once moved from the "caution" position. And although it may seem strange, it is yet a fact; the day was beautifully clear, and the signal, within easy view of the station, was not observed by either the station agent or the person who professed to be working it. It

is questionable if such could have been the case had the man had placed in front of his lever a repeater to show whether the arm rose to danger or not. The weather during the day in question had been warm in the morning, but the wind, having changed from the south-west to the north-east, was then bitterly cold. Doubtless the signal had, as was stated, been working all right during the early part of the day. To box in wires working such signals would prevent their being loaded with snow, but it would not protect them from changes of temperature, although it might help to do so; the arms themselves might be worked within a case, but they would lose their present clear and defined form, and in snow-storms the covering itself would probably accumulate the snow more than the arm, which it would obscure, and so in itself become more a source of danger than protection. Let a signal work ever so badly, if the signalman is made aware of it he has means by which he can provide against its inoperation proving a source of danger. This is effected by what is known as the "Electric Repeater."

There are two forms of repeaters, viz., that for the arm or disc, and that for the light.

The arm-repeater was introduced by Mr. W. H. Preece, who saw the necessity for it, in 1865, and tested it by temporarily applying one of his semaphore block signal instruments to a distant signal near London. A combined "arm and light" instrument, the joint invention of Mr. Preece and Mr. A. Warwick, followed shortly afterwards. Instruments for either purpose, or for the two combined, are now in use on several railways—the London and South Western, Great Northern, Lancashire and Yorkshire, Great Eastern, &c.; but their application has not been so general or so rapid as recent circumstances would appear to render desirable.

The instruments are of different forms, of different makes, and are applied to the signal itself in different ways. It is with the object of determining the principles upon which such instruments should be made, applied, and worked, that the author submits the following remarks for the consideration of the Society, and with this view it is proposed to deal with the question in the following order:—

1. The necessity for the employment of such repeaters.
2. To what signals they should be applied.
3. The nature of the indication.
4. The point of connection with the signal.
5. Form of instrument or indication.
6. Electrical construction.

Before proceeding to deal with these questions, however, it may be advisable to give a short description of an instrument and the mode in which it is applied to, and worked from, the signal-post.

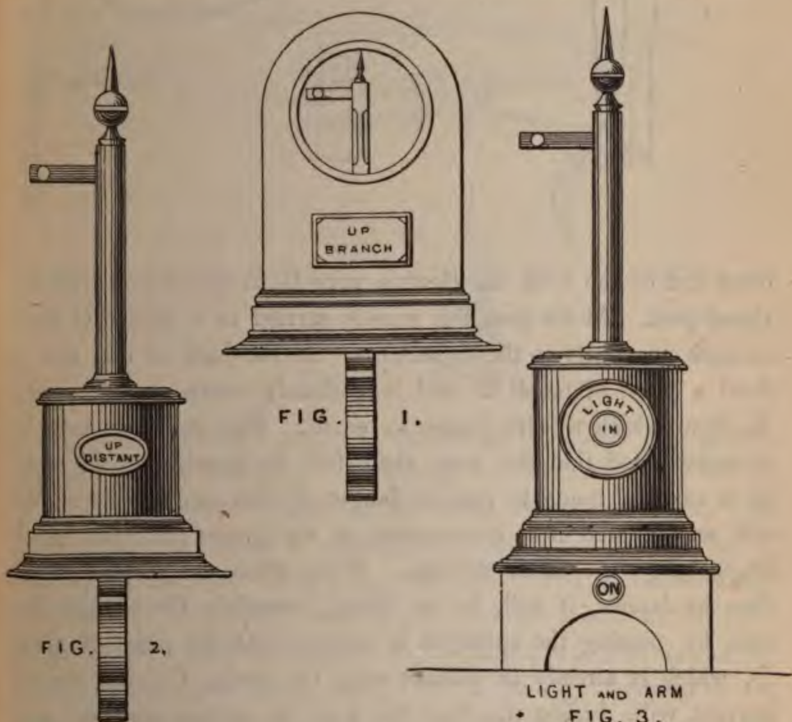


FIG. 1.

FIG. 2.

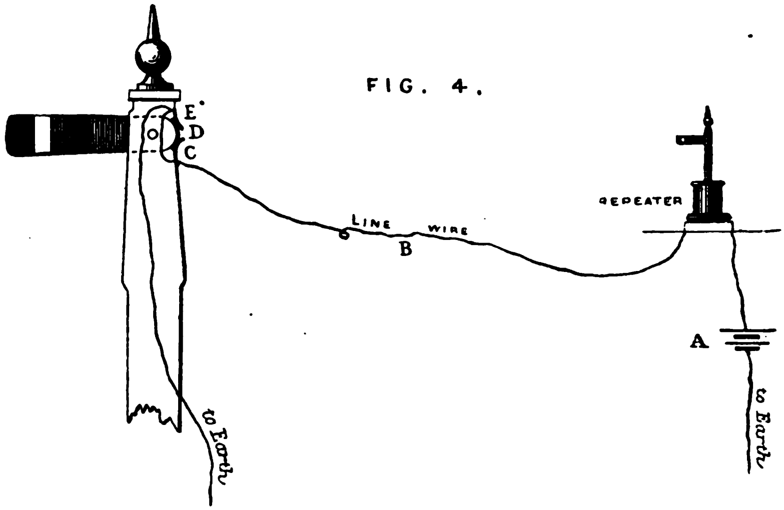
LIGHT AND ARM
FIG. 3.

Figs. 1 and 2 represent signal or arm repeaters only. Fig. 3 the arm and light combined.

Each indicator—that for the arm, or that for the light—requires an insulated wire between the signal-post and the instrument. The construction of the instruments is exceedingly simple. That for the signal consists merely of a pair of electro-magnets and an armature, from the movement of which the arm is actuated, either directly, or by a connecting rod. When a current passes through the coils the armature *is attracted, and the* arm raised or lowered as may be

required. To one end of the electro-magnetic coils is connected a battery A (fig. 4), the other pole of which is to earth, and to the

FIG. 4.



other end of the coils the electric wire B, in connection with the signal-post. At the post this wire is carried to a spring C, fixed in close proximity to the signal-arm. To the back of this arm is fixed a piece of metal D, and immediately above another spring E, from which a wire passes to earth. The repeater may be so constructed that the arm shall fall, by gravity, to all clear, or it may be made to rise to danger by the same power. We will assume that it is constructed on the former principle, which is probably the most correct. Now, when the distant signal rises to danger, it will, in so doing, complete the electric circuit, by placing the spring E in contact with the piece of metal D, which is already in contact with the spring C; the electric current will, during the time the arm is in this position, continue to flow from the battery through the coils of the repeater, along the line wire, through the contact pieces at the back of the arm, to earth. But the instant the arm of the distant signal is depressed, or leaves the perfectly horizontal position, the contact between D and C will be broken, the current will cease to flow, and the repeater will record the signal as off or *not at danger.*

To know whether the light of a signal-lamp is burning, or not, is as important, or nearly so, as to know whether the arm or the *spectacles act faithfully* or not. At night time and during fogs all

depends on the light, and the due action of the spectacles; for although the rule bids engine-drivers to regard the absence of a signal at points where such is known to exist as a danger-signal, it is likely to lead to mistakes; because, in the first place, where there is but one light the driver may overshoot the spot, and in the next place, where there are more than one, an existing light may be taken for that which has gone out.

FIG. 5.

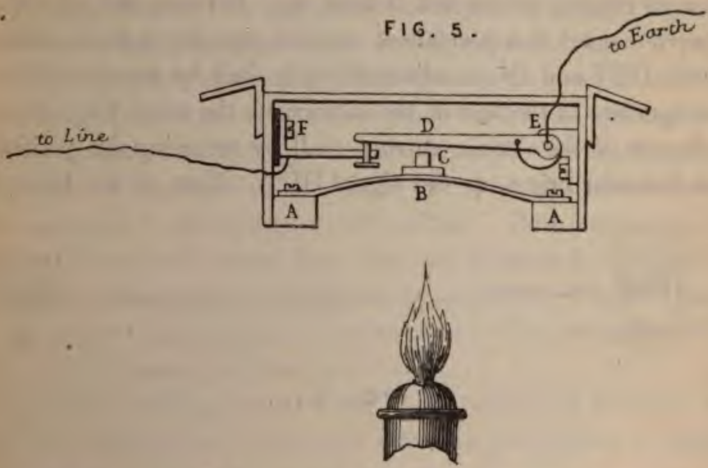


Fig. 5 will afford a means of describing the mode of registering the action of the light. Regarding it as a central cross-section of that portion employed within the lamp, A A is a circular iron frame, preferably made of cast-iron, as being less susceptible of expansion from heat. B is a piece of copper to fit A, beaten somewhat concave, and firmly fixed at its circumference to the frame A. At its centre, on the reverse side to the flame, is a stud or pin C. D is an uninsulated lever, centred at E, having imparted to it a tendency to move downwards by the spring at E. F is an insulated cock, with adjusting screw, the object of which is to regulate the space between C and D, as may be required, and to complete the electric circuit when D is in contact with it. To the insulated cock F the line-wire is joined, and the framework of the instrument is carried to earth. The diameter of the iron frame A may be about three inches, and the contact portion should be covered in to protect it from dirt. The whole may very conveniently be fitted within the top of a signal-lamp without interfering with the light or draught.

The internal arrangement of the "light" recording instrument consists of a pair of coils with an armature, to which is attached a wire rod with a bell-hammer at its extremity. Beneath the instrument is a bell-dome, so fixed that any movement of the armature shall cause it to be struck by the hammer. The line-wire is connected with the coils through a make-and-break arrangement, by which means, so long as a current flows through the wire, a continuous ringing of the bell is kept up. Between the poles of the electro-magnet is a permanent magnet carrying a shield with the words OUT and IN, so adjusted that it shall by gravity exhibit at the aperture in the face of the instrument the word IN, whilst the influence of the electric current shall by reversing the position of the indicator bring up the signal OUT. Now, if the battery be

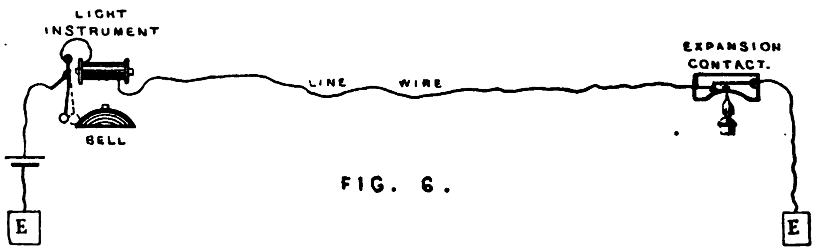


FIG. 6.

connected to the other end of the coil communicating with the "light" instrument, and its other pole be put to earth, we shall have a continuous current flowing through the coils of the instrument along the line-wire to the adjusting screw F, and through the lever D, to earth at the signal lamp. This is so when there is no light burning; but now let a light be applied. The heat will speedily expand the metal disc B, which in expanding will carry upwards the pin C, pressing it against the lever D, and finally carrying it away from the contact G. The line is then interrupted, and no current can pass. The bell ceases ringing, and the indicator being no longer under the influence of the current falls back to $\left\{ \begin{matrix} \text{LIGHT} \\ \text{IN} \end{matrix} \right\}$, and there remains so long as the influence of the heat upon B keeps up its expansion. The action is therefore this: when the light is burning the indicator shows "light in," and the bell is quiet; when the light grows dim, or goes out, the electric circuit is again completed, the indicator records "light out," and the bell continues to ring until it is restored. To the front of the instrument is fixed a switch, for the purpose of

bringing the light arrangement into circuit or not, at pleasure, as it is manifestly only required when the lamp is in use.

Reverting now to the questions at issue in the order given we come—

1.—*As to the necessity for the employment of Repeaters.*

Perhaps no more forcible argument could be advanced in favour of their employment than the recent occurrence at Abbots Ripton. A snowstorm hid the signals from the signalman's view; it prevented the signals from working efficiently, and an accident ensued. Had the signalman known that his signals did not rise to danger he could have taken steps to prevent the accident. He had his flags, his lamp, and his fog-signals; but, in the fullest belief that the signal had responded to the lever, he used neither. How many cases of this kind there are, except that they are fortunately unattended with a fatal result, it is impossible to say. Certain it is that cases do arise where the signals do not respond to the lever, and this without the knowledge of the signalman.

Fogs, rain, building constructions, the formation of the line—all these are causes which frequently intervene to prevent a signal being seen from the point whence it is worked.

The employment of electric repeaters is, moreover, a good disciplinarian. It introduces a *regularity* in the character of the signals which is otherwise absent. Let the early traveller take notice of the signals at the different stations as he passes along. Here he will see one standing well out, fully at danger; there another with an inclination towards caution, ashamed, as it were, to look the driver in the face. It has often occurred to the author that the manner in which a signal delivers its message is a very good indication of the character of the man who works it. If it comes well to danger, and again falls well to clear or caution, it is well attended, and shows a desire on the part of the man who works it to carry out his duties well and faithfully. A slovenly man is too lazy to attend to the adjustment of his wire until he is obliged to do so; but let him have before him a monitor which says to him, "Your signal is not on," when he has contented himself with throwing his lever over to danger, and his responsibility is too great for him to disregard it.

Coming now to the question as

2.—To what Signal should Repeaters be applied?

It is scarcely necessary to say their chief application must be in connection with the distant signal. Distant signals are the farthest removed from the view of the signalman, and are thus more apt to be obscured in fogs and bad weather than those nearer home. At the same time it is an open question whether starting signals and home signals, where worked away from the signal-box, should not in a similar manner be repeated back to the signal-box. It is no doubt a safe principle to consider the working of a railway under its most adverse circumstances, and to provide for it accordingly. Perhaps a dense fog is one of the worst conditions under which railway traffic can be worked. At such moments the importance of the due action of every signal cannot be over estimated. The fact that a fogman stands ready to confirm the danger signal by "detonators," does not alter the circumstances, for although, from the signal itself being obscured, it may not be so important that the signal arm shall rise fully to danger, there is yet the chance of the wire hanging in a sheave. Again, with regard to the home signal, it is not unusual, especially with junction signals, for them to be erected over the signal-box entirely away from the view of the signalman. It is not, perhaps, probable, but it is at the same time quite possible, for a disconnection to take place between the rod by which the arm is worked and the arm itself. It would doubtless be a wise proceeding if even home signals so placed at junctions were repeated, as well as those at a greater distance.

3.—The nature of the indication.

Considerable diversity of opinion exists as to the nature of the indication necessary to be recorded. Whether the repeater should simply show when the signal is at "danger"; whether it should show when it is "on" and when it is "off"; or whether it should record the three positions, "danger," "caution," and "clear," or even go beyond this, and show the intermediate positions.

In dealing with this portion of the subject it will be well to consider what is the point of danger. Clearly the point of danger is that when a signal which is intended to be ON, and which has, *so far as the* means of working it is concerned been put ON, *does not stand at danger.* It is then the man working it requires to be

told the signal is *not on*. If a signal does not stand *off* when it is supposed to have been pulled "off," no positive danger will arise. An approaching train would draw on to within sight of the home signal, and seeing this at "all clear" would proceed, the driver probably indicating to the signalman as he passed that his distant signal was not "off." With such indications and with the whistle of each succeeding train, the signalman would speedily see to the adjustment of the wire. Repeaters, to a considerable number, have been worked on this principle for some time on the London and South Western, the Lancashire and Yorkshire, and other lines, without inconvenience. Such a system requires but one wire, and the battery power is only in operation during such time as the signal itself stands at *danger*.

To show when the signal is ON and OFF, or to show when it is at "danger," "caution," and "clear," requires two wires; an application of the duplex mode of working with two batteries and a modified arrangement of the instrument; or placing the batteries at the signal-post, an objectionable course, as they would certainly freeze during the cold weather, and the electrical signal would thus become inoperative.

A method has been proposed by Mr. W. H. Preece which will overcome this, be applicable to the instruments in use, and still only consume the battery power when the signal does not stand at its lowest point, whether that be "caution" or "all clear." Such a method would require a somewhat more complicated arrangement at the signal-post, and would consequently be attended with some slight increase in first cost, but in no way such as to operate against its employment if deemed necessary.

4.—*The Point of Connection with the Signal.*

This is an important question. It is the arm which constitutes the signal by day and the "spectacles" by night. To these parts then should the connection be made. It is believed that hitherto no connection whatever has been attempted with the "spectacles," the attachment being made either to the arm, or to some part of the rod, or the lever working the arm. A moment's reflection will, however, show how desirable it is that the connection should be with both, in such a manner *that the action of the one shall be made dependent upon the action* of the other. This may be done

by means of the same wire, and with merely the additional expense of another pair of springs.

It may be argued that there can be no reason to doubt the due action of the arm if the lever has *its* due action, but there *is* the chance, although perhaps a remote one, of a bolt giving way, or the arm becoming loose on its socket, and it is desirable to provide against such chances happening at inopportune moments. It is not unknown to the author that there are signals which, from their height and construction, it would be difficult to so fit, but these are the very signals which *should be so fitted*, because it may be fairly assumed they are from those very causes less likely to meet with that careful inspection from the mechanical branch of the service so necessary to keep them in proper repair, and so avoid failure.

Disc signals should in a like manner, where possible, be fitted at the iron shaft to which the disc is fixed, and the electrical connection with these should be made as with the semaphore, through the fittings applied to the shaft carrying the lamp for the night signals.

The connection for the "light" repeater will of course in all cases be with the lamp.

5.—*The Form of Instrument, or Indication.*

As with block instruments, so with electric repeaters, various means are employed for indicating the condition of the signal. With the instrument adopted by Mr. Preece, a miniature semaphore is, as a rule, used, but other forms of miniatures of the signals to be repeated have been, and still are, employed. With Mr. Warwick an indicator or pointer has been made use of, whilst Mr. Spagnoletti uses a small shield with the words ON and OFF, which are brought up to an aperture in the face of the instrument in accordance with the movement of the arm. For the *light* record, a somewhat similar arrangement is employed by each, viz.: a shield carrying the words OUT and IN, or an indicator pointing to the same, as the case may be, whilst with the first-named indication a bell is set ringing, and continues to ring until the light is restored, or that portion of the apparatus is put out of circuit by the use of a small switch, fitted to the instrument with that object.

There is probably no great choice between the forms mentioned, *for assuming every* signalman is sufficiently educated to read the

words quoted he can make a mistake with difficulty; still there may be advantages in employing that form of instrument which accords most with the signal and requires merely a knowledge of the action of the signal to decipher a *danger* from a *clear* or *caution* signal.

6.—*Electrical Construction.*

Under this head we have to deal with the construction of the instrument, and the mode of applying the contact arrangement to the signal-post.

The instrument should be so constructed that it should be *undemagnetizable, and its signals unreversible from atmospheric causes.* Its normal condition should be that which affords the "all clear," or "caution" signal, where the signal repeated can only be lowered to that position. Its "danger signal" should be that produced by the action of the electric current.

The reasons for this will be self-evident. If the signal be not unreversible from atmospheric causes we should be apt to have an opposite indication to that required. If it is not undemagnetizable we should have a signal wanting in intensity or completeness. If its normal condition were that of *danger* instead of *all clear*, any defect in the apparatus, or the wire, would show the signal to be at danger when it might not be in that position. Constructed upon this principle, gravity may be employed to produce the *all clear* signal, and the action of the current the *danger* signal.

The system hitherto generally employed for forming the contact at the signal-post has been by means of two strong springs fixed to the signal-post, which are brought into electrical circuit with each other by means of a piece of brass, fixed to the back of the signal-arm, when it is at danger; but which are disconnected when the arm is in the least removed from that position. These springs are merely protected from the wet by a small roof or covering. It has been suggested thatthis arrangement should befurther protected from the damp air and frost; that it should, in fact, be more completely covered in, and treated as a key; but there are good reasons for the employment of this rough and ready system. The rub is a good one, better calculated to insure good contact than if the arrangement more approached that of an instrument key. It is strong, and well calculated to bear the rough action of the arm, and it can be adjusted to any nicety. Should it become coated with a film of ice

during frosty weather, a movement of the arm up and down sh
if the springs are strong enough, remove it. Still the employ
of a strong key, with a good rubbing action, might be att
with convenience in fixing and for repair.

In conclusion, with every confidence in the efficiency and p
of these instruments to perform the work required of them, si
men should be instructed, wherever a repeater is in us
*to suppose, because it does not act, that the repeater is wron
on the reverse, to conclude that it is his signal which is wrong,
take steps at once to assure himself of the contrary.*

Mr. LANGDON (supplemental to his paper) said: Mr. Pres
I am glad to have this opportunity of bringing under the cons
tion of this Society a subject which I feel to be of importanc
only to railway companies but also to railway travellers.
can be no question that if railway signals are necessary at
the protection of railway traffic it is equally necessary some
should be obtained in order to show that they work as the
intended to work. It may be advanced as an argument ag
the employment of electric repeater signals that there are
railways on which they have never been used, the traffic on
has been conducted hitherto with perfect safety, and in such a
ner as to show that there is no want whatever of repetition t
signal; but the same might have been argued with regard t
signals recently used on the Great Northern line at Abbots Ri
had not the trains there followed in close proximity one on the o
Had there been a greater interval between them, in all probal
the accident would not have arisen; and the same might be u
in other ways. Unless signals for the government of railway t
can be made infallible, a repetition of some kind is certainly ne
sary in the present state of increased railway traffic. But it ma
further argued that electric repeaters have already established th
selves. Such, no doubt, is the fact with regard to *some* of our railw
but even in those cases it is only the fact to a very partial ext
Electric repeaters have been employed only in isolated places
under special conditions. For instance—where the signals

away from the signalman's view entirely, or perhaps at some important junction where some mishap may have occurred, or where something may have arisen to shake the confidence of the railway manager in the signals at that point.

In the paper which I have read I have not attempted to deal in detail with the several forms of instruments that are used for this purpose; neither have I sought to explain the means of applying them to the signals which are required to be repeated; for I have thought that an opportunity such as this would be embraced by all those interested in the question to discuss these points, and to bring before the Society such appliances as were thought fit for the occasion; and that it would be better to leave to those interested in them the explanation of their arrangement, electrical and otherwise. I am sorry I have been so unfortunate with regard to the instruments, which ought to have arrived this afternoon, as otherwise I should have been able to have given a more practical illustration of the action of the signals, and a better description, perhaps, of the electrical formation of the instrument. I do not think a more opportune occasion could have occurred for the discussion of this question than the present. The railway mind has been brought to look upon it in a very forcible manner. We, ourselves, are here under the presidency of one who has spent no small portion of his life in the application of electricity to railway working; and, no doubt, whatever decision it may lead to, the result will be looked upon by railway companies as having been fairly fought out and dealt with by railway men.

THE PRESIDENT: You have heard this paper read. It must have reminded some of you of a couplet in Homer's *Iliad*, thus translated:—

> A wise physician, skilled our wounds to heal,
> Is more than armies in the common-weal.

I mean, that electricity is now so pressed into the service of railways, that not only is safety mainly dependent upon it; but it keeps them in a wholesome state of health. Trains, for instance, are kept apart by its use, but it heals the wounds and accidents of their system; if a casualty occurs, the responsibility for softening the blow rests with those who have the charge of the electrical apparatus. If inconveniences occur within a train when travelling, electricity is responsible if the passenger fails in getting the atten-

tion of the guard, or if the guard fails in getting the attention of the engine-driver. If the distant signals fail to obey the levers of the signalman, electricity is responsible to tell the tale. In fact, the more you look into railways the more you will see and continue to see that with us railway telegraph engineers a large weight of responsibility rests. Whatever goes wrong, we are called into requisition. Should an engine break down, or defects of any kind occur on the railway itself, and should electricity fail to reach the electro-magnets and galvanometers, and do its full duty, the electric telegraph department has to bear the burden of failure. But, also, what a very great friend electricity is to the travelling public! Of this I, myself, can personally bear witness. I have now to call upon gentlemen present, who, I have no doubt, will be glad to speak upon this attractive and very interesting subject.

Mr. HIGGINS: I believe it is better to have two wires to work the signal mechanically instead of one wire, as well as the telegraph. This would have the advantage of requiring less strain to lift the danger signal, and would also indicate when the wire was broken; it could only go wrong at any time to the extent of putting the danger signal at "caution." The plan which I recommend would be to have a drum in connection with the semaphore arms, or the rods which lift them, and a corresponding drum at the signal-box. A wire, endless or not, should then be fastened with staples to the drums. Any movements of this drum at the signal-box will produce a corresponding movement of the drum at the signal-post. In case the wire break the signalman would see by that wire not being pulled out that the signal had not responded to his wish. When a wire breaks between the time of the signal being put on and being required to be put off, that could not affect the trains, because the signal only waits the signalman's pleasure to change, and as soon as he wishes to change if the wire is broken he would discover that by the broken wire not acting. Snow could not affect the arm if worked in this manner, because in the ordinary arrangement the semaphore arm is pulled up by the counterpoise weight, and the counterpoise is lifted by pulling a wire. In this case there is no counterpoise to move, and the wire on that account would be a little lighter than the other.

Mr. SACH: I feel personally much obliged to Mr. Langdon for

bringing this subject forward, as it is one of great interest to telegraph engineers. Although I think the railway telegraph engineers and railway companies are fully alive to the value of electric repeaters, and have been for several years, I am an advocate for their use—they are already in operation on an extensive scale. I myself have some 200 to 250 in use, and am adding to them every week. I rather object to Mr. Langdon's view of having the battery inside the signal-box. I think it is much better to have it at the post, for the signalman then knows he gets the direct and proper action of the current; there is more liability to error if it is in the signal-box, for instance, in a closed circuit, with the arm at danger, and a wire breaking with its end on the ground, the repeater would still show at danger, although the arm at the signal point might be at safety. There are other chances of mistake also by the battery being at the same end with the repeater.

Then there is the matter of making contact. I have tried various methods of contact, and I find those exposed to the weather in all cases get defective. I think every contact should be enclosed—cased in and protected from the weather—then you can insure a good contact at all times. I have a specimen of contact here. This is acted upon from below, and being so it enables you to case the whole in and hermetically seal it. I have here a piece of No. 8 wire, extending to the under part of the arm, and when the arm is lowered this lowers with it, and the lowering of the arm breaks the contact, and the raising of the arm makes contact. The action is direct from the arm itself, and not depending upon a rod or any other apparatus. I have used contacts of various kinds, and I find that the contacts require protection from the weather.

Mr. Varley: What metal do you use for the contact?

Mr. Sach: You may use gun or any other metal. I am using galvanized iron now, and have not had a single failure.

Mr. Langdon: How long have you used batteries at the posts, and do you not find they freeze in cold weather?

The President: Perhaps it will be more convenient to answer these questions at the end of the discussion.

Mr. Sach: I may say that batteries will freeze without some little preventive; but, by making a rough sort of shell and placing a lining of felt between it and the outer box here is an effectual remedy; we have adopted this plan with complete success.

Mr. E. T. Rolls: I have derived much pleasure from Mr. Langdon's paper, seeing that the subject is of so much importance in railway working. I quite endorse Mr. Langdon's remarks upon the varying tension of the signal wire, and can go even further, for I know it to be a fact that upon a day of intermittent sunshine the length of the wire varies with the passing clouds.

Some experience in the maintenance of repeaters has convinced me that for various reasons contacts are best encased and protected from the weather.

First, because in frosty weather, just when fogs are most likely to make repeaters of vital necessity, an exposed brass plate frequently acquires an insulating envelope of ice, and I do not find that springs flexible enough to be safe from breakage will scrape their way through this glacial envelope, and then signalmen are apt to get into the way of saying, "Oh, it's the frost."

Secondly, by exposure to moisture chemical and sometimes electrolytic action will frequently coat the exposed plates with salts of copper from the brass.

As to the nature of the indication I am of opinion that the repeater should certainly show the two positions of "off" and "on." By making the current actuate both signals, a double check is obtained; the working battery may be thereby more rapidly consumed, but this has not been found objectionable in some block systems.

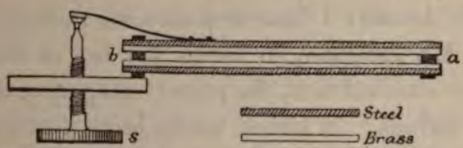
Further, such a plan would be more satisfactory to most of the men, who sometimes complain that in regulating the wire to suit the "on" repeater signal they have nothing to check them against overdoing it and making the wire too slack to get it off.

To obviate the exposed contacts, and register both positions of the arm, using only one wire, I would place working upon the same axis as the arm and well cased a 4-way switch, which would show the signal to be on or off, as the case may be.

The battery must be kept by the signal-post, and so far from being a disadvantage I consider this a gain; with a stout kennel covered with roofing-felt, an excellent non-conductor of heat, frost need not be feared. I have had batteries exposed under such conditions through the last two severe winters, but no failure occurred. At the worst a hand-lamp could be introduced to warm the little battery hut, as is sometimes done in the signal-box battery cup-

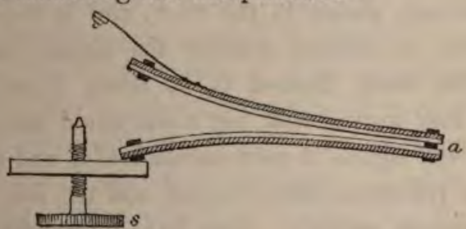
board—there, with more reason, three sides being usually only protected from the air by thin matchboarding sometimes not devoid of open chinks. The advantage I claim is that the lineman must go to the signal itself to attend to the battery, and then he will probably examine the contacts. But with the battery in the signal-box and the contacts on a signal perhaps three-quarters of a mile away, a lineman in dirty weather is apt to shirk the work of making a journey to the signal and back.

As to the light repeater. I have had working for some time an apparatus consisting of two compound plates arranged thus:—



The steel of the top-plate is on the upper side, the brass at the bottom. The lower plate has the steel underneath; they are both rivetted together at one end *a*, and at the other end *b*, *one* plate is fixed to the side of the lamps. S is a contact-screw insulated from the rest of the apparatus.

Under the influence of heat the plates curve in opposite directions, and the lower composite plate also tips the top one upwards, adding this motion to that obtained by the curvature by heat, and the two plates assume something like this position:—



Thus without the assistance of any multiplying lever or other device of the kind a very wide motion is obtained direct from the source, so affording great nicety of adjustment by the insulated contact-screw, and a good rubbing-contact.

Two of these repeaters have been at work for a considerable time without failure; as far as I have any knowledge, for more than two years without a hitch; one was attached to a lamp with a varying gas-supply, and invariably rang up the signalman for more gas when it sunk *too low*, as it sometimes did in the small hours.

All these light-repeaters require a wire for their separate use, but it has just occurred to me that by placing in circuit a tube of selenium in front of lamps showing white and red and having also in circuit a balanced resistance, the disturbance of this balance by the varying action upon the selenium of the actinic white rays and non-actinic red would enable a single wire to record by day the position of the arm, the spectacle by night, and the condition of the light.

I have placed one of the light-repeaters on the table, and shall be happy after the discussion to explain its working more fully.

Mr. A. J. S. ADAMS: I have seen on some railways the repeater described by Mr. Langdon, in which the bar or rod moving the arm has a plate attached to it, the plate passing up or down between two insulated springs. It struck me as being possible for the correcting-bolt to slip and allow the bar to slide without moving the arm; therefore I think the proper place for the contact is between the arm itself and a metal contact. I have watched some of these repeaters on the Great Eastern Railway for three winters, and they have always presented clean contact-surfaces.

Mr. C. E. SPAGNOLETTI: With regard to signal-repeaters, which Mr. Langdon's useful paper has brought before us, the first I tried was on the Metropolitan Railway, in 1863. When that line was first opened it was required to work signals from one station to the next station in the rear; and as the distance was great it became a question whether the signals should not be repeated. This being decided to be done, I put up several repeaters and they worked well, but it was found the distance was too great for the signals to work satisfactorily, and consequently they, with the repeaters, were removed and the line was worked on my block system entirely. Some have lately been fixed on the Great Western Railway on the principle of which I have a model here. This system shows the position of the arm, when it is on, when it is off, and when it is wrong, *i.e.*, a doubtful position between a certain and unmistakeable signal, because it is possible where you have only one contact which most electricians use, if the arm is just below the contact-point the indicator would show the signal was down, whereas the signal might be mistaken by the driver for being up, and it would be the cause of delay as well as *check the train.* The system is excessively simple, which is a most

essential requisite, and the apparatus is fixed where it is easily got at to clean if required, and to be looked at at all times. The spindle which works the signal-arm is squared at one end, and on this end there is a little metal key; this is boxed in, and the box covering it contains two springs; one is the zinc pole of the battery and the other the copper, and the spindle itself forms earth. The line-wire is attached to the centre of the battery, and the current is sent from either pole as the key on the signal-spindle is worked, and by the one battery you get the double movement. The battery is placed in a box at the post, and from its being in this position you know the signal you get on the instrument must come from that post, whereas if the battery is in the signal-box accidents can and do happen by which the signal may by the instrument be showing as working all right when it is really not working properly. The box in which the battery is, is made with a double casing, filled up or packed with sawdust. This is sufficient to prevent freezing in a severe frost, and we have never had any trouble with boxes of this description. This system only requires one wire, because by the reversal of the current we get two signals, and by the persistent current which costs very little extra you always get an immediate register on the disc instruments if any accident should happen to the wire, instruments, or battery. The lamp indicator we are using is simply a mixed metal bar of brass and steel, so arranged that the expansion shall be increased by a lever action, and I find we can with one of these expanders get contact in 10 seconds, and it takes 15 seconds to show the contact off again. The great difficulty I find with the lamp expander is the variation of the burning of the lamp. If oil is used it burns for many hours, but towards the morning the flame gets low, either from bad oil, want of oil, or carbonized wick, and then the signal "lamp out" is shown, which is right practically; although the lamp is not really out, it is burning too low to give a good and clear signal. When gas is used we find when the pressure is turned off towards midnight the reduced pressure affects it and its results are varying, and I have found it necessary to arrange them, that the contact when the lamp is in shall be made with a minimum amount of light sufficient to give a good

and clear signal, and however much more heat you may get, and the expander increase on its expansion, contact shall be kept up between this minimum point and the maximum point of expansion, no matter how it varies between these two points; but directly it contracts sufficiently to get below the minimum point then contact is broken and the lamp is shown as out, which for practical purposes it may considered to be. The indicator I use is one of my ordinary disc instruments; one disc recording the movement of the signal arm, whether off, on, or wrong, the other disc recording the burning of the lamp, showing lamp in or out. In connection with the light indicator there is a switch; the man lights the lamp, and when the disc shows "light in," he then turns the switch handle to bell on, and brings the bell in circuit. If the lamp goes out the contact made with the disc is broken, and it falls to "lamp out," and the bell at once commences to ring, calling the attention of the man to the fact at once; if the switch is turned off the bell stops ringing, but the disc still shows "lamp out" until the lamp is relit. The bell affords an excellent means of testing the instrument at any time during the day. You have only to turn the switch-handle to "bell on" to see that the instrument and battery is in good order. This is an economical system to use, and economy is a necessary thing, because if these repeaters are very largely used, which no doubt they will be, their cost will form a considerable item. Mr. Higgins has called attention to night to the double wire system: that is a very good system, being one of the first, and was introduced by Mr. Brunel on the Great Western Railway; in fact I believe it was his own signal, and was worked with a double wire, but although the signal may go to danger, yet if the wire breaks you have no indication of the fact that the wire has become broken in the signal-box unless the lever was moved.

The PRESIDENT then announced that, as the hour for closing had arrived, the discussion on this paper would be adjourned until the next meeting.

The Forty-seventh Ordinary General Meeting was held on Wednesday, the 12th April, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The preliminary business of the meeting having been transacted,

The PRESIDENT said: At the last meeting Mr. Langdon read a paper on "Electric Repeaters," the discusion upon which was commenced. It now devolves upon Mr. Langdon to reply, unless, indeed, other members may desire to make further remarks upon the question or upon the instruments produced. In that case we shall be pleased to hear such remarks.

Mr. F. RUDALL: I believe, Sir, visitors are allowed to take part in the discussions? If so I would say, after about ten years' experience of electric distant signal indicators or repeaters on the London Chatham and Dover Railway, that the conclusion at which we have arrived is to secure three objects, viz.:

First, that the source of the electric current shall be at the distant signal itself; the latter in fact may be regarded as an automatic block signal station, and it appears to me that no one would think of sending the current from one station for the signalman at the distant station to use in replying. The source of the current should be at the signalling station itself. Again you want to retain a clear space between the distant signal and the box, and therefore it is desirable that the source of power should be at the signal itself. I may add that we have our batteries boxed in to protect them from frost.

The second point is, that the position of the semaphore arm when at danger shall be shown by a constant positive current, which moves a suspended needle to point to the words "signal on," and the position of the arm when at safety shall be shown by a constant negative current, which reverses the needle, so as to make it point to the words "signal off;" in fact, ours is a one wire

system, worked by means of a reversed current, positive for danger and negative for safety.

The third point is, that when the semaphore arm either fails to reach absolute danger, or to be lowered to absolute safety, the current is broken and the needle assumes a perpendicular position.

The ingenious idea of employing gravity to indicate one of the positions was abandoned, because while the semaphore arm itself might fail to act properly the indicator would only show this as to one of the two positions; and although the telegraph wire might be broken and the electric circuit destroyed, the indicator in the signal-box would still give its gravity reading. The above is a summary of the method we have found it desirable to adopt. Our Engineer is most exacting in having a constant current always at work. There is, of course, a considerable expenditure of battery-power, but then the signalman knows positively that while the current is working the signal itself must be pointing either to "danger" or to "safety," as the case may be. We have had these instruments eight or ten years in work, and we find they give very good indications. The only difficulty has been where the rod has not acted properly, and I quite think the current should be sent, as in Mr. Spagnoletti's model, by means of the arm itself instead of by the signal rod, as is done in the case of Mr. Sach's ingenious contact apparatus. That is all the light I can throw upon the matter.

Mr. Langdon: I shall be glad, if not thought trespassing, to redeem my pledge with reference to the exhibition of the instruments on the table. I unfortunately on the last occasion missed obtaining the requisite apparatus to illustrate my paper. That apparatus is now here. (Mr. Langdon here described the apparatus which had been placed on the table, and which consisted of model distant signals, repeaters of various forms, and expansion pieces, the application of which was practically illustrated.)

In addressing myself to this subject I have endeavoured to confine my remarks as much as possible to the points which are most important to obtain an efficient repetition of distant and other signals which require to be repeated. Of course there are many modes of effecting this; but if we introduce an erroneous system,

however well it may work for a time it is certain in the end to produce failure, attended, it may be, with some disastrous effect. It is only a question whether the failure of the signal should be attended by the failure of other equally important checks. I may, perhaps, instance this by some rough notes which have been placed in the hands of the Secretary by Mr. Graves of the North Eastern Railway, in which he states that he employs his electric instruments so arranged as to stand normally at danger—that is, the arm being normally in a horizontal position; and he says if these signals had been at work at Abbots Ripton they would have shown the signals there were not on. This, no doubt, is true, provided the contact arrangements and the wires were all in proper working order; but those are points which we cannot always insure: a wire may break, or a battery may fail. In that case I think Mr. Graves is somewhat premature in saying instruments so arranged would have shown that the distant signals were *not on*, because the signal, standing by gravity normally at danger would have shown the signal was *on*.

I have stated it is desirable we should ascertain as far as possible to what signals electric repeaters should be applied. I am sorry to find that is a point which has not received the attention it deserves. It is one of the most important points, and it would have been of great advantage if we had obtained an expression of opinion of some railway men on the subject. Perhaps, however, I may be allowed to offer a few suggestions; and I would say, in the first place, that it is necessary that repeater signals should be applied to *every signal which is out of the view of the man who works it.* I am not now speaking of foggy weather, but of ordinary occasions, when a man may be prevented from seeing a signal either by its being placed overhead or from some obstruction on the line. An instance occurred yesterday at Clapham Junction, where the South Western Company has erected a large and handsome signal box, and where some of the signals are in close proximity to the box. One of the signals became detached from the rod which worked it, consequently the arm did not work but stood at "clear," and a train, governed by it, was about to leave the station while shunting was going on, but was prevented by the foreman of the yard, who

fortunately noticed the state of the signal. In this case the signal was within 10 or 12 yards of the man working it, and yet he was unable to tell whether it was working properly or not. I would next say that distant signals should be repeated in all cases *where the block is in use or the traffic is heavy.* Wherever the block is in use trains are run at rapid rates and at short intervals. Wherever this is the case there is too little margin for failure; the failure of a signal may lead to fatal consequences. If it is necessary to divide lines of railway into block sections to keep the trains apart, it is necessary that the signals should act with perfect accuracy, because if they do not the object of the block is entirely lost: *the distant signal is the key of every section.*

With regard to the nature of the indication some variety of opinion exists. Some require only one indication and others require three. Mr. Rudall prefers to have three signals, and Mr. Sach also, but in the paper I have given reasons for requiring only one signal. I regard the question as one which should be discussed, more particularly by railway men, who know their own requirements best. All I desire to point out is, that with the existing systems of repeaters, where two or three indications are required, two wires must be used or the battery must be placed at the signal post. Mr. Rudall says he places his battery at the post. I would ask him whether he has found any inconvenience arising from cold weather? Mr. Sach also stated he places his battery at the signal-post. I have myself had some experience in using batteries placed out of doors. When—some years ago—block signals were being put up on the South Western Railway, means were wanting for placing the batteries in some of the signal boxes, and they were consequently enclosed in cases lined with straw. Yet the frost attacked the batteries. I feel satisfied batteries placed in such positions must, during a severe winter, become congealed and rendered inoperative. I therefore still strongly advocate placing the batteries in the signal-box instead of at the post.

I have in the paper referred to a plan which Mr. Preece has devised for working a repeater from the distant signal by means of *one wire*, with *one battery placed in the signal-box*, and which shall give *three or more indications*, as may be required. I am sorry

Mr. Preece is not here or has not commissioned some one to explain to the meeting the means by which he proposes to effect this, but it is perfectly practicable, and it is one of the safest means I know of obtaining those indications at a low cost. It is evident, if you use the battery at the signal-post, the battery must be always at work.

With regard to the connection at the signal-post, we have had no reference to the suggestion thrown out that it should also be extended to the spectacles which form the signal by night. It is a very important question, and I think all railway telegraph engineers would do well to give it consideration, because there is equally as much danger attending the working of the traffic at night as in the daytime, and therefore if it is necessary to repeat the action of the arm it is equally necessary to repeat the action of the spectacles. As to electrical construction, the general impression seems to be that the contact arrangement at the back of the arm is the best, but that it should be covered in or protected from the weather. Mr. Goldstone, of the South Western Railway, has been kind enough to send me an arrangement which he has employed for this purpose, and he intends to alter all his contact arrangements to the same plan. It is a very neat contrivance and is worthy of inspection.

I feel that I cannot conclude without referring to the remarks which fell from the President with respect to the application of electricity to railway working on the occasion of the last meeting. He then stated it is usually only when every other effort has been tried that the telegraph branch is had recourse to and generally with success. This I can quite endorse, and I think any person who has a knowledge of railway working can readily understand it. The telegraph branch of our railway system has not received that confidence which it merits. This is perhaps due to one or two causes. In the first place it is probably owing to the fact that railway managers know but little about electricity, and consequently have not so much reliance on it as upon mechanical appliances, and which are more evident to the eye.

The large field open to the economical and advantageous employment of electricity in railway working must however ere long be

brought forcibly before every railway management, and there can be no doubt that the day is fast approaching when the telegraph branch of all our large railway systems will hold a position second to none in the service.

The PRESIDENT: I think we are much indebted to the author of this paper for bringing before the Society a subject of this very interesting kind; and I am sure that before we pass on to the next paper you will agree with me, and gladly, to give him your thanks. The subject which has come before this Society will go out of this room into the railway world, and will doubtless attract the attention of railway companies towards this useful application of electricity more forcibly than it has hitherto done. The telegraph engineers attached to the various railways are quite able to comply with all reasonable instructions that may be given to them, with the view of providing any electrical arrangements that may be required; but I do not go quite so far as Mr. Langdon in thinking that electricity should be made too responsible for some things. There are few things in reason beyond our grasp, beyond our power. But we must not too readily transfer to ourselves and accept the responsibility of carelessness on the part of other people. This introduction of electric repeaters to the distant signals of the railway, the South Eastern, to which I am attached, is a very old question with us. We had instructions some years ago to apply electricity to distant signals. We applied it; and are continuing week by week, and as fast as we can, to extend the application in divers directions. Our impression is, that electric repeaters shall show (1) when a signal is up and (2) when it is down, and also (3) when the green and red spectacles each in part are visible; the signal being doubtful, that is, in a false position. I believe that was the position of the signals at Abbots Ripton on the night of the collision. I would say further, that the railway telegraph engineers are ready to meet requirements put before them for still more complicated applications of electricity. It is only to be hoped they will not be asked to be too much responsible. Really one sometimes thinks we are almost to be responsible for everything else that fails, electricity on a railway being in practice the one being assumed to be infallible. If Mr. Langdon and the other

railway telegraph engineers, myself included, will so arrange matters as to make the failures very few indeed, which I have no doubt the majority of them will do, we shall continue to be, as we have heretofore been, of very great service to the railway interest of this country. I will now ask you to express your thanks to Mr. Langdon in the usual way.

The vote of thanks having been unanimously awarded, the following Paper was then read:—

THE WORK-VALUE OF ELECTRO-MAGNETS ENCLOSED IN IRON.

By CHARLES V. WALKER, F.R.S., F.R.A.S., President.

At the Conversazione held in Willis's Rooms by Mr. Latimer Clarke, our past President, on Dec. 21, 1875, among the exhibits were "No. 76, Altandæ Electro-Magnets," by Mr. John Faulkner, which were described in the circular that he placed in our hands as "Faulkner's Altandi Systemæ.—The Altandæ system depends upon a peculiar electric phenomenon, whereby one coil of an electro-magnet and of less size produces a greater amount of magnetism with less battery power than two coils on the old system." The word Altandi or Altandæ, in the absence of explanation of its origin, was not inviting; it conveyed no idea to one's mind; but more magnetism with a lesser electro-magnet and from less electricity was very attractive; it was something that could be dealt with. Attention was further called to this system by *The Telegraph Journal*, of 1876, Jan. 15, in a report, taken from the *Manchester Courier*, of 1875, Dec. 1, of a lecture delivered at the Manchester Literary and Philosophical Society, by Prof. Osborne Reynolds, M.A., in which the lecturer informed his audience that "Mr. Faulkner has some magnets of this kind, which retain the keep with 100 times more force when the outer tube is on than when it is removed." And, in further confirmation of the value of magnets of this form, a letter from Mr. Ladd, the well-known optician, &c. of Beak Street, appears in *The Telegraph Journal* of 1876, Feb. 15: "I have constructed electro-magnets on this principle for the last twelve years. The following

is one way in which I have applied them: in the 'Daboscy form' of electric lamps, an electro-magnet is employed in connection with the clock-work, which regulates the carbon point. With a small battery I found the ordinary forms of electro-magnets had not sufficient power over the keeper to obtain the necessary increase. *I enclosed the electro-magnet in an iron tube, connecting it at the base with the centre tube or bar*, and surmounted the outer tube with an iron flange. This enabled the poles to act with considerable power on the keeper." The words in *italics* contain the fundamental principles on which these magnets are constructed. In fact, an ordinary straight electro-magnet becomes a horse-shoe or U-magnet, and with its polar extremities more than twice as near to each other as they can be by any ordinary arrangement, and the armature practically one-half shorter. This is seen at a glance on fig. 1, where *a a′* is the iron core; the full line, *b b′*, a section of the iron cylinder and of the iron disc or bar that joins it to the back end of the core *a a′*; the dotted line *c c′* is a similar section on the opposite side of the cylinder, and so on all round; so that, if the front end N of the core acquires north polarity under the action of the current, the back end S of the core would have acquired south polarity, but which will now be shown at the front ends S′ S″ of the cylinder. But the current at the same time would be inducing south polarity at the front ends S′ S″ of the cylinder, which *mutatis mutandis* would make the front end N of the core still more full of north polarity. This appears to be the *rationale* of the action of these magnets.

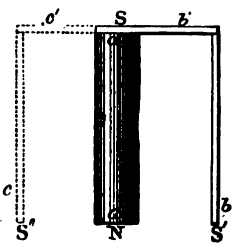


Fig. 1.

An electro-magnet of this kind was long since figured and thus described by Mr. D. Davis, of Boston, U.S.:—

"In figs. 2 and 3 are represented, at *a a′*, two double cylinders of iron, enclosing a coil. A sectional view of one of them is given separately at A (fig. 3). It contains a cavity in the form of a hollow cylinder, adapted to receive one-half of the coil seen at C (fig. 4), the remaining half passing into the other cylinder when they are fitted together. A longitudinal opening on one side of

the cylinders allows the wires of the coil to pass out. In this arrangement the cylinders adhere with great force when in contact; but as soon as the current ceases their magnetism instantly disappears, and the adhesion does not continue as with the semicircles.

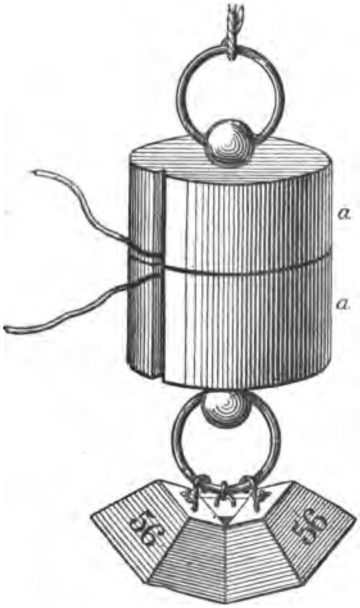


Fig. 2.

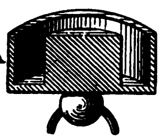
A 

Fig. 3.

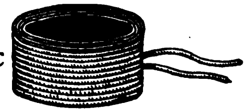
C 

Fig. 4.

When one of the cylinders is passed over the coil, the part exterior to the coil exhibits no sensible polarity." *

The arrangement before us differs from this in that in the one case, seeing that no motion is required from the armature, the latter in size, weight, and arrangement is equal with and a counterpart of the enclosing cylinder; but in the other, the case before us, where motion is required, and not mere adhesion and lifting power, the section between case and armature is not midway, but at one end,

* *Manual of Magnetism*, Second edition, p. 171, fig. 111. Boston, U.S. 1848.

flush in fact with the acting pole of the core of the electro-magnet, which in this instance is exposed, and free to attract a moveable armature, that is adjusted, when at rest, to be retained by a spring at a given working distance from the core, and protected by banking-pins from coming into actual contact with the core, when at work.

In text-books that are accessible to me while writing, I have not found any description nor any mention made of electro-magnets of this construction. On the other hand, I find the following passage in Count de Moncel's book :—" Un noyau de fer enveloppé par une hélice voltaïque s'aimente énergiquement; alors qu'un cylindre de fer enveloppant cette même hélice ne s'aimente pas de tout."*

In the system with which I am most familiar, for spacing trains to avoid one overtaking another, and for other kindred purposes for increasing the comfort of, and ensuring safety to, Railway travellers, no less than fourteen hundred and odd electro-magnets are in use, and month by month this number increases, all being of the horse-shoe or U form. A system which promises, under like conditions, to give more work out of one-half of the electro-magnet in ordinary use than we now obtain from the entire magnet, could not fail to attract one's attention, and constrain one to put it to the test—cost, bulk, weight, all promising to be reduced.

The aspect under which I have attacked this question, as you see, is purely utilitarian. I selected for the comparative trials one of the bells in ordinary use on the locomotives and in the guards' vans on the South Eastern Railway, fig. 5. It is on the table before you. The bobbins are each $3\frac{1}{16}$ in. long by $3\frac{1}{16}$ in. diameter, and are wound each with 308 feet of No. 18 copper wire, which weighs 35 oz.—or in all, 616 feet, weighing 70 oz. The iron cores are $\frac{5}{8}$ in. in diameter, each weighing $3\frac{1}{2}$ oz. connected at the back in the usual way by an iron bar. The distance from core to core, *a a′*, shown on the front of the magnet, is $2\frac{1}{2}$ inches. But face-plates of iron, *b b′*, screwed upon the front of the cores, bring the distance between the poles down to $1\frac{3}{16}$ inch. The armature

* *Recherches sur les meilleures conditions de construction des Electro-Aiments.* Par le Comte Du Moncel, p. 110. Paris, 1871.

$c\,c'$, shown in dotted lines, is $3\frac{1}{2}$ inches $\times$ $\frac{1}{2}$ inch $\times$ $\frac{1}{9}$ inch. The length of the hammer stem $d\,d'$ from the line of the centre of motion $e\,e'$ to the point f, to which the cord, hereafter to be mentioned, is attached, is $6\frac{1}{2}$ inches in all the experiments. This, and all the figures that follow, are drawn one-third of the full size.

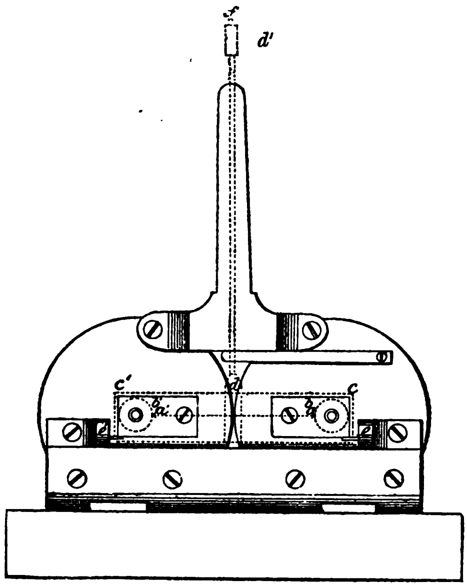


Fig. 5.

The practical result to which we desired to arrive was the impact of the hammer on the bell, that is, the amount of sound to be had out of a given number of battery cells from this or that form of electro-magnet. To this end it was enough for all practical purposes to measure the attraction between the armatures and the magnets. A cord was attached to the point f on the hammer stem, which passed horizontally over a pulley on the same level, and descended vertically to a hook or to a scale-pan. The battery current was turned on, and weights were added until the armature was released. It is hardly necessary to mention that the armature was not in any case allowed to come into actual contact with the iron face of the magnets, but was kept back by banking-pins, in the usual way, with working apparatus.

We now took one of the coils from another bell, similar to and

in all respects identical with fig. 5; and removed the face-plat b b' from the front of the core. An iron cheek was substituted f the original brass one at the back of the bobbin; but this seen hardly necessary. The iron tube, shown in section in fig. 1, ar in back view in fig. 6, was fitted to encase the coil. It w attached to the back of the core by a screw at a, and came u flush with the front. Its weight was $16\frac{3}{4}$ ounces; heavier, possibl

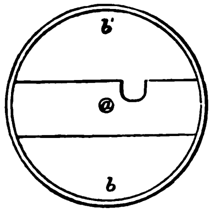


Fig. 6.

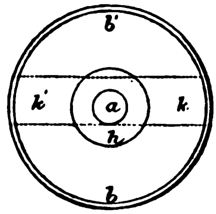


Fig. 7.

in proportion to the core than was necessary. The front of t coil thus prepared is shown in fig. 7, where a is the core, $1\frac{1}{4}$ in distant from the tube b b'.

Fig. 8 is the front of the encased bell, where b b' is the tube; a

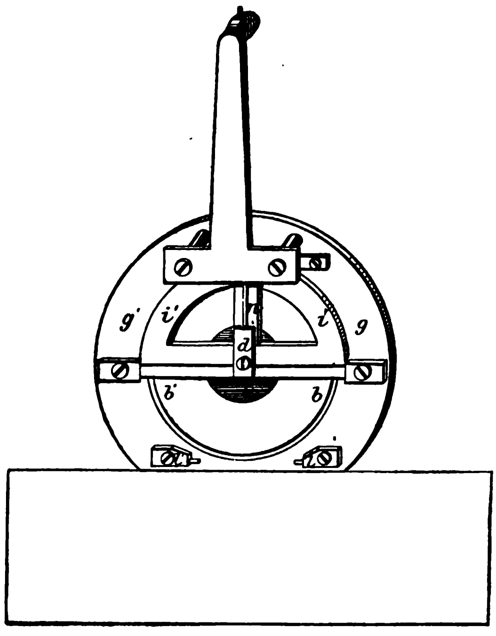


Fig. 8.

g g' is a brass flange on which the fittings are mounted; *h* is a disc, and *i i'* an armature, to which reference will be made hereafter.

The battery employed was platinized graphite and zinc; the plates 6 inches × 2 inches; the exciting liquid, 1 sulph. ac. + 8 water.

With three cells on short circuit, the encased magnet, fig. 8, held 1·91 oz., the magnet of the train bell 4 oz., or in the proportion of 1 to 2·09. An iron disc (*h*), 1¼ inch diameter, was now countersunk in the brass face of the coil, figs. 7 and 8, which reduced the distance between the two poles of the magnet from 1¼ inch to ⅞ inch. The magnet now held 3·39 oz., as against 4 oz. as before of the train bell, or in the proportion of 1 to 1·18, being still not quite equal to the magnet of the train bell.

Two armatures were now prepared, in the form of open segments of a circle, of the diameter of the iron cylinder, or 3¼ inches. They are shown in figs. 9 and 10. The upper segment, fig. 9, is to be carried by the same cocks that carry the plain

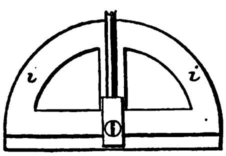


Fig 9.

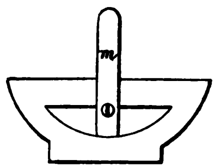


Fig. 10.

armature *k k'*, fig. 7, and is of like length—3½ inches; it is ⅜ inch wide and $\frac{1}{9}$ inch thick, and weighs 2 oz. The lower segment, fig. 10, has its centre of motion below at the cocks *l l'*, fig. 8; its length, $3\frac{5}{16}$ inches; width, ⅜ inch; thickness, $\frac{1}{9}$ inch; weight, 1½ oz.

With the same three cells on short circuit, and the disc *in sitú*, the lower segment held 14·70 oz., or in the proportion of 3·67 to 1 compared with the train bell magnet.

The upper segment held 17·80 oz., or in the proportion of 4·45 to 1.

With both segments on, only 13·25 oz. were held, or 3·31 to 1.*

* The finger *m* on fig. 10 touches the upper armature at *n*, fig. 8, and acts with it.

With or without the disc the comparative figures were :—

TABLE I.

	Without disc. oz.	With disc. oz.	Ratio.
Plain armature - -	1·91	3·39	1 to 1·77
Both „ - -	10·00	13·25	1 „ 1·32
Lower „ - -	[1·15]	14·70	[1] „ 12·78
Upper „ - -	10·95	17·80	1 „ 1·62

The apparent irregularity of the lower segment armature, without the disc, is explained by the fact of its upper edge not reaching so high as the lower point of the iron core. From this it is evident that the maximum value is to be had from a coil cased in iron, with the front end of the core expanded, and a segment armature mounted as shown in fig. 8.

The relative value of the armatures in the above Table is :—

TABLE II.

	With disc.	Without disc.
Plain armature - - - - -	1·00	1·00
Both „ - - - - -	3·84	5·23
Lower „ - - - - -	4·32	[0·60]
Upper „ - - - - -	5·25	5·73

The relative effect of outside resistance on the armatures respectively when the disc is used, still retaining three cells, is given in the following Table :—

TABLE III.

				Ratios.		
	2 R	R	0 R	2 R	R	0 R
Plain armature - -	0·93	1·53	3·39	1	1·63	3·51
Both - - - - -	7·81	10·06	13·25	1	1·28	1·69
Lower - - - - -	7·03	8·90	14·70	1	1·26	2·09
Upper - - - - -	9·08	12·33	17·80	1	1·35	1·96
Train-bell - - -	1·66	2·10	4·00	1	1·20	2·46

The resistance R in all cases was a single coil of No. 18 copper wire 308 feet in length, similar to the bell-coils.

The relative effect of outside resistance upon the armatures, each with the other, is given in the following Table:—

TABLE IV.

	2 R	R	0 R
Plain armature	1·00	1·00	1·00
Both	7·55	5·81	3·84
Lower	8·39	6·57	4·32
Upper	9·75	8·03	5·25

The practical result of this inquiry is—

1st. That a single coil cased in iron, in the manner described, gives nearly as much work-value as a pair of coils, each fitted up in the more usual way.

2nd. That one encased coil with a segment-armature, having its centre of motion at or nearly in a line with the diameter of the coil, gives about four and a half times as much work-value as an ordinary pair of coils.

3rd. That the work-value of a magnet with core and disc of the dimensions given is one and a half times greater with the disc than without.

4th. That the plain armature in the encased coil is less affected thus far by resistance than the upper or the lower segments; and so in the train-bell.

A single uncased coil is of no practical value side by side with a cased coil. It holds 0·5 oz. only. The cased coil, according to the armature used and with the disc, holding 3·39 oz., 13·25 oz., 14·70 oz., 17·80 oz. So that the best form before us of cased coil gives thirty-five and a half times as much work-value as does a plain coil.

I have shown that with three cells the cased coil, with upper segment and in short circuit, held nearly three and three-quarter times as much as the train-bell. With ten cells it held nearly five and a-half times as much, the figures being 4·95 oz. and 27 oz.; the train-bell rising from 4 to 4·95 oz., or as 1 to 1·23; the cased bell from 17·80 oz. to 27 oz., or as 1 to 1·51.

Table V. is a summary of the weights held by the armature in the several arrangements, and of the relative value of the respective combinations, showing also the comparative value, a single encased coil being taken as the unit. They are arranged in groups, but numbered in order of value.

TABLE V.

WITH THREE PLATINIZED GRAPHITE CELLS.

	In Short Circuit.				Ounces.	Relative Values.
1	One train-bell coil		- - - - - - -		0·5	1·00
6	Cased coil.	Plain armature	- -	No disc	1·91	3·82
15	„	Both „	- -	„	10·00	20·00
3	„	Lower „	- -	„	1·15	2·30
17	„	Upper „	- -	„	10·95	21·90
8	Cased coil.	Plain armature	- -	With disc	3·39	6·78
19	„	Both „	- -	„	13·25	26·50
20	„	Lower „	- -	„	14·70	29·40
21	„	Upper „	- -	„	17·80	35·60
9	Train-bell complete		- - - - - - -		4·00	8·00
4	Cased coil.	Plain armature	1 coil R,	with disc	1·53	3·06
16	„	Both „	1 „	„	10·06	20·12
13	„	Lower „	1 „	„	8·90	17·80
18	„	Upper „	1 „	„	12·33	24·66
7	Train-bell complete		1 „		2·10	4·20
2	Cased coil.	Plain armature	2 coils R,	with disc	0·93	1·86
12	„	Both „	2 „	„	7·81	15·62
11	„	Lower „	2 „	„	7·03	14·06
14	„	Upper „	2 „	„	9·08	18·16
5	Train-bell complete		2 „		1·66	3·32
	With 10 Cells. No Resistance.					
22	Cased coil.	Upper armature	- -	With disc	27·00	54·00
10	Train-bell complete		- - - - - - -		4·95	9·90

APPENDIX.

Among the experiments made were the comparative value of an ordinary train-bell, with or without cased coils. The proportion was as 1 to 1·6 in favour of the cases, which is not much.

A single cased coil, compared with a pair of coils cased, and arranged as a train-bell, was as 1 to 1·22. So that a single coil gives nearly the same work as does a pair. In these cases the armature was in one piece of the proper length.

When the armature was in two pieces, that is, when a single armature was given for each coil and coupled into one by a brass strap, the value fell considerably. Compared with the whole armature it was as 1 to 1·91.

I made but one trial of a cased coil, with a second coil of equal length outside the case. The result was of a negative character. The ratio between the value of the magnet, with and without the second coil, was as 1 to 2·28. But Mr. Faulkner, I believe, has carried this question more fully out, and had the proportion of iron and wire so adjusted as to obtain a larger increase of power from given materials than one could hope for from a solitary experiment. This question is worth pursuing by any one who has more leisure at his command than I can pretend to claim.

March 29, 1876.

The PRESIDENT: You have heard this short communication of mine read, and you can see at a glance that it is the production of a very busy man, who has had time only just to touch upon the threshhold of a subject, which must be interesting to telegraph engineers, inasmuch as it is a means of getting much more than the ordinary amount of work out of the same materials. This, so far as I am aware, is the first record of experiments made upon magnets of this kind. The principle, it occurs to me, is very old. During the short interval since it first attracted my attention I have referred to the various text-books I have in my library, and

have searched among communications made to societies—not exhaustively. Records may be in existence, and some of our members may be able to give still more of the history of this curious magnet. Personally I have not yet met with much information. Mr. Ladd is present, to whom I refer as having used the form of magnet to which I have alluded. His magnet is lying upon the table, and he will no doubt give us all the information he possesses upon the value of this class of magnets and the use he has made of them.

Mr. LADD: I have but little to say in addition to the President's elaborate paper. I apprehend that, although a busy man, he has more time for experimenting than I, who make instruments, have at my disposal. I have been very much interested in the results of his experiments with the particular form of magnet under discussion. When I saw the title of the paper read this evening, it occurred to me that an opportunity might offer for repeating what I had reason to write in the Telegraph Journal a short time since, concerning the use I have made of the iron-cased electro-magnet for the last twelve years. From what I have since learned, I believe this description of electro-magnet had been made before it occurred to me to adopt the principle; but although new to me then, I did not set forth a claim of anything new. I was, and am now, satisfied with the result obtained.

The magnet I have brought for your inspection is one I took this morning from an old electric lamp, and it is the form I have generally used; the centre is cylindrical, and is made so for the free passage of another part of the instrument to which it belongs; this cylinder or core is joined to the outer cylindrical case by a flat iron ring, firmly uniting the outer and inner cylinders and forming the bottom of the magnet; on the upper edge of the outer cylinder I fix another flat ring or flange, the inner diameter of which is made to come within a quarter of an inch of the central cylinder; thus the poles of the magnet are brought very near together. This condition is necessary for the particular work required of the magnet; also that the wire used should be stout, say No. 9, and consequently of short length. I tried a few rough experiments this morning with the encased magnet, of a nature calculated to

determine its practical value, and found it able to support a weight of 8 or 9 lbs., the weight being held by a disc not more than three-quarters of an inch in diameter, whereas the same magnet uncased barely supported its own weight (about 2 lbs.) A battery of 2 Groves' cells was used in these experiments.

The PRESIDENT: Mr. Faulkner is present who exhibited a collection of magnets made in this manner at Mr. Latimer Clark's Conversazione, and I am sure we shall all be glad to hear anything which he is disposed to communicate.

Mr. JOHN FAULKNER said: I am not accustomed to lecturing, but I may say I have been accustomed to showing experiments, and I hope any want of words will be made up by the experiments which I will perform before you.

The paper before us is as the title states, "The Work-Value of Electro-Magnets enclosed in Iron." I exhibited, as many of you are aware, at the last Conversazione, a number of instruments illustrating what I have chosen to call the Altandæ system, and at that time I distributed a very modest circular stating that "the Altandæ system depends on a peculiar electric phenomenon, whereby one coil of an electro-magnet and of less size produces a greater amount of magnetism with less battery power than two coils in the old system." If we can by any means make two blades of grass grow where only one grew before, we do good. The Altandæ magnet means literally "a raised or exalted magnet," and in that sense it is so applied in the illustrations which I shall give.

I have here a bar of iron possessing no magnetism. I place upon it a bobbin containing wire, through which I send an electric current, and the consequence is magnetism is set up, and to distinguish that from the ordinary magnet we call it the electro-magnet. I will now send an electric current through the coil, which I have here prepared. The core is of iron, one inch thick, and it is surrounded with wire in the ordinary manner. I will send a current through the wire and electro-magnetism is produced. I now put on a thin sheeting of iron (about No. 16), and you see that a remarkable change comes over this electro-magnet. It would not lift its own weight before; now it lifts its own weight and adheres with considerable force.

I now put over the magnet a thicker iron cylinder, and the weight now is held with a force that no man in the room can overcome. This is the Altandæ electro-magnet, and I am sure it is deserving of the name.

Here I have a magnet of another kind—it is larger and I have greater effects from it. I have another here still heavier and with a thicker cover, and this brings us to what we may call the maximum amount of magnetism that can possibly be obtained from the quantity of electricity employed, and that is a point of great importance, as we utilize that which has hitherto been wasted.

Up to the present time we have only, under ordinary circumstances, used one per cent. of the available power of electro-magnetism. You see on the wall a number of diagrams. These are produced by scattering iron filings over the poles either of an electro-magnet or Altandæ electro-magnet, and this (exhibiting) is the diagram so obtained. The scattered rays represent 2·97 lbs. waste out of a total of 3 lbs. Here we have a diagram from an Altandæ electro-magnet. You perceive there are no scattered rays at all; within the cover the force lies, and what would otherwise be waste rays are all gathered up, and, as you see, into a small focus. I have another diagram taken from the outside of the Altandæ electro-magnet, and in that we have no rays of force outside. The only rays of force we find are at the two extremities.

Having illustrated the action of iron filings under various conditions of the Altandæ electro-magnet, Mr. Faulkner went on to say: I would suggest to students of magnetism, that if they would only follow out the course I have taken with regard to these diagrams, showing the lines of force, they will find very great assistance from employing them. When I have been in the greatest despair, I have referred to diagrams and they have relieved me from the difficulty.

With regard to the covers made use of by our President, he has produced 35½ times more power by using this thin covering; had he used a thicker he would have succeeded in obtaining still greater power. (Mr. Faulkner further illustrated the effects of the Altandæ magnet by means of iron filings sprinkled on paper placed over the

magnet, showing the effects of the addition of the iron covering in concentrating the rays of force with apparently scarcely any appreciable waste of power).

There is another quality which comes into force. You take an electro-magnet formed of wire only. You know a coil of wire with an electric current passing through it has magnetism on one side of a different character to what it has on the other. The rays that are on the right side may be called north and those on the left south. What is done by the covers is, they lead all those rays which go forth into space on the right to the right, and those on the left to the left and so intensify the action. If I now send an electric current through the coil (one end is a north pole and the other a south pole, whether the covers are on or not) that shows that the electrical rays are really what we call magnetism. Those which go to the right we may call the north and those which go to the left we may call the south. These terms will probably have to be modified in future mention of the working in connection with this subject.

Having shown you how this comes about, the next thing is to show you what I have done in practice. The first thing I did was, to take an uncased magnet from one of the best descriptions of bell-magnets, to strip off half the wire from one coil, put it into this Altandæ box, and I got a better bell-magnet with a quarter of the coil I had before. I have another variety from which I have got very excellent results, and which has a nicety of finish about it which is not to be found in the ordinary electro-magnet, by means of which you are able to apply the Altandæ magnet in a space in which you cannot apply the ordinary electro-magnet. Moreover we can place them on a plane; one plane of 22½ inches square will give 81,000 lbs. of force. Here I have an Altandæ magnet with an armature on the top of it; I make out of it a telegraph sounder—as also an Altandæ indicator and Altandæ semaphore indicator, each actuated in the ordinary way, with which you are all acquainted.

The next and one of the most important points is the regulating of the power we have. I have here an Altandæ telegraph sounder. The beauty of this instrument is that we can regulate the power of the Altandæ magnet. You saw when I put on a cover I increased

the power, and so when I want to regulate a given amount of power, I have not to consult the battery but the arrangements I have in the receiving instrument. We have here an ordinary electro-magnet in a brass case and over that a surrounding cylinder of iron. Suppose your battery weakens, or you are sending a long distance and your battery power is not equal to the work, all you have to do is to introduce more of the cylinder. You have here the means of regulating to a considerable extent without additional battery power. Again, suppose after fine weather a heavy shower comes, and everything gets wet and the instrument works sluggishly, screw the cylinder further in.

I have great hopes of this little instrument, one of the latest productions, and it is the best of them all. I have a moving bobbin working on an iron core, and I can assure you this is one of the most honest electrical instruments in existence. Here I have only one iron cylinder and an electro-magnet inside. I have already shown you that the iron covering concentrates the power, and the closer the cylinders are to the coil the stronger is the power; therefore by moving the inner cylinder out we can open the field and thus regulate the power to the required strength.

These Altandæ electro-magnets can be applied, as you see, to various purposes, and the great number of purposes to which they can be applied is my excuse for obtruding a new name into the business of the manufacturing electrician and into science.

I found that, not only may we use round covers, but square ones. In an instrument I made I had occasion, for the purpose of separating iron from brass filings, to place my magnet against a square-opening. I found I had a round force to work in a square. Of course that would not fit, so I constructed a square one. I will show you the diagram (here a three-eight iron plate was placed on each side of the electro-magnet). This shows you the way in which the square plate operates. You perceive where there is iron all the rays are harnessed, so to speak, and brought into the space between the plates and the core; at each open side they are bursting out and wasting themselves in space. You thus see, by using these covers we not only get out of an electro-magnet more power than has before been obtained, but we can also put the force where we require it, and can

apply it to any description of instrument whether round or square.

I have one other little experiment which is very interesting. (Mr. Faulkner then exhibited the tracing of the word "Manchester" on a sheet of paper placed over the magnet, formed by scattering iron filings over the sheet.)

In conclusion, I will only say I have done my best to make myself understood, and if I have not been quite clear on some points, I shall be happy to give any further explanations at the close of the meeting.

The PRESIDENT: I am sure we are much indebted to Mr. Faulkner for the interesting instruments he has brought and the description he has given of them. No doubt other gentlemen in the room would like to make some remarks upon this form of magnet, but, as we have passed the time at which we usually close our meetings, we will postpone the further discussion till our next meeting on the 26th instant.

Adjourned accordingly.

The Forty-eighth Ordinary General Meeting was held on Wednesday, the 26th April, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

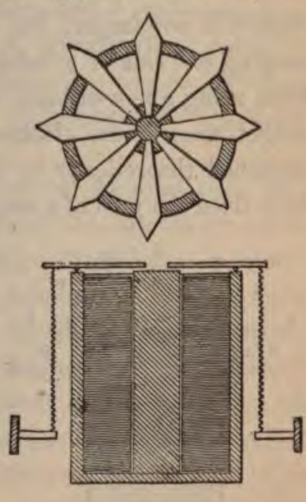
The PRESIDENT having called upon any gentleman who had further remarks to make upon the paper read at the previous meeting, on "The Work-Value of Electro-Magnets Enclosed in Iron," to do so,

Mr. C. F. VARLEY said:—I have a few remarks to offer, and but very few. In 1855 I made a magnet of this description for apparatus placed at Amsterdam, but I found that although when the armature was attached to the magnet I got greater lifting-power, yet at a distance from the magnet the power was less than that of the ordinary horse-shoe magnet. I also experimented with what are called in France *aimants boiteux:* that is to say, an electro-magnet with one arm covered with wire and the other one free. It is quite true that with such a magnet you get greater lifting-power from a given current than might be expected, but for telegraph work you want more than lifting-power; you want rapidity of action above all things, and I found this form to be slower in magnetising and demagnetising than the ordinary electro-magnet. For telegraph purposes I gave it up; and you will see by reference to a patent of mine in 1856 mention made of electro-magnets covered with iron, but they did not answer as well as the forms subsequently adopted and now in general use.

Mr. HIGGINS: I have tried a number of experiments with electro-magnets, and shall be happy to give the results. They led me to make a form of magnet which differs totally from that which Mr. Faulkner has brought before us. The electro-magnet covered with iron I saw in use about fourteen years ago in a relay constructed by Mr. Varley. That worked strongly but was extremely slow from the magnetism having to impregnate the outer casing and

pass through a long distance from the bottom armature of the magnet. These magnets have been used in France for separating iron and brass filings. The electro-magnets used in the Exchange Telegraph Company's instruments are tubes bored out of solid iron a little more than one-third of their diameter, and then split through to the centre to prevent circulation of induced currents. In the magnet of Mr. Faulkner the outer casing favours the circulation of induced currents. It may be split to prevent that; but in its present form it must retard the magnetisation of the core. Even supposing the tube to be split there would still be charges induced on the outer coating by the high tension of the inner and outer spirals of wire of the magnet, which, at the moment of breaking battery-contact, would have some effect in retarding the magnetisation and demagnetisation of the magnet. The diminution of the field of the magnet would no doubt reduce its effective force to some extent. The armatures of the magnets used in our instruments will respond to 3,000 currents per minute, and will do a considerable amount of work. I am quite sure a magnet encased in iron could not possibly do that.

Mr. Saunders: I might just mention that a magnet (of the shape shown in the accompanying figure), exceedingly small in form, was made by Mr. Whitehouse, in 1857, at the workshops of the old Atlantic Telegraph Company. It is of exactly the same form as that of Mr. Faulkner's.



The object, however, was not to get an increase of power, but to arrange the poles of a magnet so that as many armatures as possible could be acted on; this was done by arranging them as shown in the sketch.

In the instrument designed by me for Mr Whitehouse the armatures were as close together as possible, and about double the number shown in the sketch. The bobbin was wound with No. 36 wire; and the object of the instrument was to show the curve of an

electro-magnetic current on chemical paper by the decomposition of steel needles.

Each armature was insulated and moved on two pins resting on the outside iron cylinder. The force with which they were kept from the inside bar of iron was regulated by adjustable spiral springs. And this force was gradually increased from a minimum applied to one armature to a maximum applied to the east of the circle, so that the armatures were attracted in rotation as the current gradually increased in force.

Each armature was connected with a steel needle, and on being attracted made contact, so that the needle left a mark on the chemical paper which was rotating on a drum.

Mr. Treuenfeld: I agree with the experience of Mr. Varley that though an encased electro-magnet shows considerably greater power for keeping the armature, yet that power is much diminished when the armature is at a distance from the electro-magnet. I think for telegraph purposes it bears out the suggestion that the work-value cannot be taken when the armature is close to the electro-magnet, but only when it is at a certain distance, and I venture to say that when the armature is at a distance from the electro-magnet the working value of the encased magnet is not larger, but smaller. This can easily be tried by hanging one side of a balanced scale over the electro-magnet, and when using a non-encased electro-magnet it will raise a greater weight than if the electro-magnet were enclosed. This may be explained theoretically by considering that the lines of magnetic force from the pole of a magnet go in radial directions to all sides.

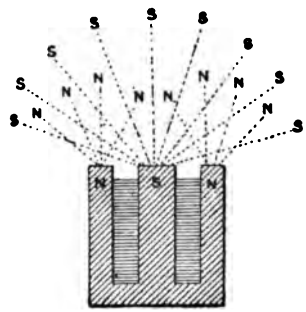


Fig. 1.

Now supposing an encased electro-magnet, with the armature at a certain distance, we then have the south pole in the centre, and a circular-shaped north pole formed round it. Both poles have radial lines of force, and at a distance these lines cross and destroy each other, which is not the case in a non-encased magnet. If the armature is a certain distance from the encased magnet, the

force gets diminished by the lines crossing and destroying one another, therefore a non-encased magnet has greater attractive power than an encased one. A reference to the accompanying diagram will show the action which takes place very clearly.

Mr. ADAMS: In the year 1862, being in Turin, I saw this kind of magnet used for relays, and, being curious about it, I made some experiments with the idea of finding a very rapid magnet. I found these magnets, though they had very great holding power, were not so rapid as the ordinary magnet, and the results with regard to correct time-rate were exceedingly bad. The Government carried out some experiments for long lines and weak currents. When the current was limited they had to diminish the iron, consequently there was no great force. I can only agree with Mr. Varley that for rapid currents this form of magnet is not equal to the ordinary magnets.

Professor FOSTER: I came across a little while ago a description of a magnet of precisely the construction which has been referred to—a tubular magnet; an iron core with the usual coil surrounding it, and a plate of soft iron at one end with a wrought-iron tube incasing the coil and fitting close to the plate. Such magnets seem to have been made by a good many different investigators; but the first reference to them that I came across is in a paper by Römershausen, who gave a description of them in 1850. He found that in a small magnet, the size of which I forget, the lifting power was increased sixty-four-fold by putting a wrought-iron tube round it.

It is quite obvious, as has been pointed out by more than one speaker, that at a distance from the poles the attracting force would be diminished by this arrangement; but while it is so we have increased lifting power, when the keeper is in contact with the magnet. Previous to this magnet by Römershausen, about 1840, a magnet of another kind was introduced by Mr. Richard Roberts, of Manchester, which may be seen in the exhibition of scientific apparatus now open. This was made by cutting out a circular groove, or rather two or three concentric grooves, out of a flattish piece of iron about three inches in diameter, and it is stated that it will lift a weight of 1,400 or 1,500 lbs. which I think is a

great performance for so small a magnet. Roberts also constructed magnets in which the grooves, instead of being circular were rectangular, crossing the face from one side to the other.

Mr. HIGGINS: In the magnet to which I have already referred the performance did not differ from others, except that the speed of the armature was very great. The armature was made of a number of rays from an iron axis. There were two sets of rays from this iron axis which revolved in front of the poles of a series of electro-magnets placed parallel with the axis carrying the armatures. These were magnetised in succession. Three at a time were always magnetised. With a thermo-pile burning ten feet of gas an hour, this motor would lift 40 lbs. weight one foot high per minute.

Mr. DARKIN: I represent Mr. Comacho, the inventor of the tubular magnet, and with reference to that magnet I beg to say that I have found the very reverse of the action referred to by Mr. Varley and others, that is to say, at a distance the magnet gives a considerably greater result than one of the ordinary construction, while its power at contact is not so remarkable. I have a large specimen of this magnet at our office, and shall be happy to let any gentleman see it. It will lift 600 lbs. a distance of a quarter of an inch and drop it again forty times a minute with a battery of six Bunsen cells. It consists of concentric tubes.

Mr. VARLEY: If I understand, this magnet contains two or three concentric iron rings [Mr. DARKIN: Four] and the power is applied in one direction. It is not a magnet of the description under discussion.

Mr. DARKIN: No. I gather it is something similar to the one you constructed.

Mr. VARLEY: The magnet which Mr. Darkin speaks of is of a different character. It consists first of a hollow iron core; then round it there is a certain amount of wire. Outside of that is an iron tube with a further quantity of wire, and outside that another tube with a further amount of wire and yet another iron tube, making the fourth, with a much larger quantity of iron wire. I have not experimented with that magnet; but, forming an opinion without experiment, I should say if all the iron was put into one solid core you would get a better result than you get now.

Mr. DARKIN: Would you get the same result at a distance?

Mr. VARLEY: I believe so.

Mr. F. C. WEBB mentioned, that in experiments made by M. Du Moncel with the Comacho magnet, referred to by Mr. Darkin, the results of each successive coil were tried by itself and afterwards added together, and then the whole series was tried together, and it was found that the four cylinders together produced a lifting power of twice as much as the sum of the four taken separately. The experiments were carefully made, and in each case the resistance of the circuit was the same, and the same current passed in each case. That was to his mind conclusive as to the value of these compound magnets.

MAJOR MALCOLM: I fear this Society has no power to regulate the names which the inventors of new instruments give to them, and I sympathise with the remarks which have been made as to the barbarous term which has been concocted for the instrument under consideration. I have made inquiries among gentlemen of far greater classical pretensions than myself, and they agree with me that the name is utterly barbarous, and I think it will be a pity if the term "Altandæ system" is perpetuated; still I suppose there is no law to prevent it. To a certain extent I have made experiments with this apparatus, and I am very glad to hear what Mr. Von Treuenfeld and others have said, for at first I was afraid that I had misunderstood the invention and was experimenting wrongly. I tried it in the following way: I had a core which was 14½ inches long, and a cover about 14 inches; I hung a small steel ball by a very long thread in front of it, and I found that the drawing power of the magnet with the cover on was less than it was with the cover off—except in one position, viz. when the cover was very nearly off. Nevertheless, as to holding power, I have no doubt from experiments which I am satisfied are accurate, if the cover be put on, and if you have an iron base connecting the cover with the magnetized core, the holding power is enormously increased, although it is also very great if there be no such connection. Therefore, while I can give Mr. Faulkner no credit for the name, I think he is deserving of great credit for bringing the instrument before us, many uses for which will probably soon develop themselves. I

represent more or less the requirements of the military, and we want sometimes to know what you telegraph gentlemen are saying; if then we can tap your line with portable instruments of low resistance we shall have a better chance of knowing what is going along the line than if high resistance instruments are used. It is sometimes a matter of great importance to us to know what is passing along lines, and we look upon your wires as things to be tapped, therefore we are desirous that Mr. Faulkner and others should be encouraged in the production of sounders, &c. of great sensitiveness and of low resistance; and if they will only name them according to some known law of language they shall have my best wishes. May they go on and prosper.

Mr. LADD: I regret that I was called upon to speak at the last meeting before I had heard the remarks of Mr. Faulkner. No doubt these enclosed magnets are of great use for many purposes. In the case in which I used them, I obtained more power than I wanted, and had to reduce the quantity of wire in order to get the lesser magnetism for my purpose. Now, if those who heard Mr. Faulkner at the last meeting left with the idea that what he said about cased magnets was correct, they will have some very erroneous impressions. A great deal of what he said is—I was going to say—"Altandæ" nonsense; for instance, that the outer casing is something only that prevents waste of magnetism, and it has no magnetism itself. This we know is wrong; the outer casing forms one of the poles of what I have always called an annular magnet.

Mr. JOHN FAULKNER: I have taken the liberty of distributing amongst the gentlemen present this evening sheets of diagrams illustrating the various appliances of what I have chosen to call the Altandæ system. Upon those diagrams you find first of all a diagram showing the large amount of waste of power there is in the common electro-magnet. No. 2 is a diagram showing the economy of force there is in the Altandæ magnet. We say there is waste in No. 1, because it can only lift 1 or 3; but in the form of No. 2 diagram it will lift 100 or 300, and I am sanguine enough to believe we can get more than 100-fold of increase by a properly arranged Altandæ. Having got this great force, as I illustrated the other night, I will describe what I applied it to in the first instance.

In making common electric bells we have two coils, and what I did was to take one of these coils reduced in size, cast away one-half of the coil, and subsequently three-quarters of the coil, and I then got better results with a quarter of the coil than I did with the two original coils. Finding that to be the case my thought turned to the applying of it to any bells and sounders. I went on with experiments, and I was encouraged to a degree to determine that I would no longer waste electricity by making use of it to make an electro-magnet, but that I would rather raise the power of the electro-magnet by means of the iron cover. Having done that for the purpose of bells, &c. I conceived that we might regulate the power, and I found that the thickness of the cover had a great influence on the complete realisation of the power. You will remember in my illustrations at the last meeting I enclosed the electro-magnet in a thin cylinder, and when I scattered the iron filings on the paper you saw that still waste of force took place, but when I put a thicker covering over that actually ceased and the power was entirely in the centre and not outside; if I had a heavier covering still it would have produced more effect. The importance of this comes to us in other ways as we can alter the field. No. 7 on the diagram is what I call an Altandæ telegraph sounder, with regulating cover. Here we have the cover outside moved by a screw, and as we screw the cylinder on we gain in power. We are told this cylinder will no increase the effects of a small quantity of electricity. Indeed it was to exalt the smaller quantity that first directed my thoughts in this direction. Having made an instrument that would work by putting on more tube instead of more battery power, I conceived that was a beneficial thing. I have given diagrams on the bottom of the sheet, which I describe as "other examples of regulating telegraph sounder, (*a*) by expanding the field, (*b*) by moving the core, and (*c*) by moving the coil." As for the statements of the various gentlemen to-night, I assure you my experience is that it is as quick if not quicker in action than the ordinary electro-magnet, and when properly arranged at a distance where the force exists it is better for all practical purposes than the common electro-magnet. I thank you for the kind attention you gave me the other evening, and if I have in any way been the means of

assisting those who are thinking something of the matter, or if I have given new thoughts to those who have never thought of the importance of it, my time will not have been misemployed. You can all see I have thoroughly gone into the matter, not by one or two tests but by hundreds, of which these diagrams are the proofs.

If I had a little more time I could show you a phenomenon in this matter which will become of great importance and which I have no doubt you will hear more of in the future.

Briefly it is this: I have observed in diagrams taken from the open side of an Altandæ electro-magnet with flat iron sides, that the curves of central magnetism or the neutral point changed with the thickness or quality of iron placed across one or both ends. From this suggestion I have made an instrument, which is on exhibition at the Loan Exhibition of Scientific Instruments, in the South Kensington Museum, for testing the thickness or quality of iron plates in boilers, faults in telegraph wire, &c. &c. I called it the Altandæ Electro-Magnetic Iron Plate and Wire Tester. This is shown in fig. 2.

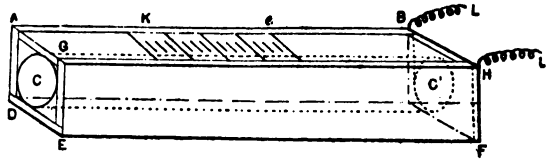


Fig. 2.

A F is an Altandæ magnet encased below and on the two sides with iron plates; above is a brass top A G H B, on which a scale *k e* is graduated. C C′ is the core situated in the centre and passing lengthwise through the magnet; round this is wound a coil of wire, from the terminals of which the wires L and L′ lead off to a battery. When the instrument is to be used, the face A D E G of the magnet is placed against the iron plate to be tested. A small compass is then passed along the scale *k e* until the neutral point is found. The whole is then moved along the surface of the plate in various directions, and any change of the neutral point in the magnet is at once observed by means of the dipping of the compass-needle. The thickness of the plate may then be known by observing the distance along the scale between the neutral point when the

magnet is placed against the plate, and when it is removed; any defect in the thickness of the plate is likewise at once observed.

The PRESIDENT: I am much pleased at having called attention to an electro-magnet, which appears to have been but little known, although first described some thirty years ago. Since my communication was written I have made further experiments, and have tried armatures of various forms, six in all, and in a different position as to the centre of motion, which in all the present cases was in a line with the bottom of the coil. It will be seen that the tracing out the behaviour of these coils with various armatures is very interesting, and well worth investigating, altogether apart from their practical application. The results of my further experiments are as follows:—

When we used the plain armature like $k k'$ (fig. 7), but fixed vertically, and with its centre of motion at one end, as shown in fig. 11, the attraction rose to 43 oz. with 3 plat-graphite cells, and to 20 oz. with 1 Leclanché.

Fig. 11.

Using two such plain armatures at right angles to each other, as shown in fig. 12, in fact a cross, the attraction showed a further rise, reaching to 48 and 26 oz. respectively with the 3 plat-graphite and 1 Leclanché cell.

With a complete disc, as shown in fig. 13, the attraction, which with the open disc or wheel had been 32, was exactly doubled, and became 64 oz., or 4 lbs. with the 3 plat-graphite cells, and 31 oz. with the 1 Leclanché.

Fig. 12.

In these six experiments the core was $\frac{10}{8}$-inch iron; in figs. 5 and 8, in the earlier experiments, it had been half this size, or $\frac{5}{8}$-inch, the size of the coil and its iron case being in each instance the same, but one holding of course a few yards less of wire than the others.

Fig. 13.

I found that our Mr. Stelges had taken the armature $i i'$, fig. 8, from its position there, and had dropped it down and made its centre of motion at the bottom of the coil, and so that the *top* of the segment faced the iron core, as seen in fig. 14. With three plat-graphite cells the armature held $19\frac{3}{4}$ oz., and $13\frac{1}{2}$ oz. with the single Leclanché.

Fig. 14.

It was thought now, by inverting this same armature,

so that the *top* of the segment was downward, and the straight edge on a level with and facing the iron core, fig. 15, that the attraction would increase, seeing that more surface of armature and magnet would be presented to each other. The reverse was the case: it fell from $19\frac{3}{4}$ and $13\frac{1}{2}$ to 16 and $9\frac{1}{2}$ oz.

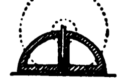


Fig. 15.

When a whole circle armature open like the segments, a wheel in fact, fig. 16, with four spokes or radii, was used, the attraction rose from $19\frac{3}{4}$ and $13\frac{1}{2}$ to 32 and 17 oz.

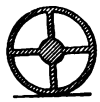


Fig. 16.

The magnet with the larger iron core with its lesser length of wire gave just about the same attraction as did that with the smaller core, and the greater length of wire, when tried with a single Leclanché cell.

It remains to determine, each for himself, the cases in which this form of electro-magnet can be advantageously substituted for the ordinary U-shaped; regard being had to the relation between the resistance external to the magnet and that of the magnet itself.

With these remarks I close this discussion, and will now call upon our Acting-Secretary to give the Meeting a few words of description of Edison's "Electric Pen," the working of which is exhibited here to night by a gentleman who has been kind enough to attend for that purpose.

Mr. Sivewright in describing the Electric Pen said:—

We are indebted to the patentee in England of Edison's Electric Pen, Mr. Thomas Clare, of Handsworth, near Birmingham, who most readily complied with my request to come here this evening and exhibit its working to the members of the Society.

The object of this pen is to pierce fine holes in sheets of paper, forming stencils, from which impressions are taken. This is done, when the stencil is once prepared, by passing an inked roller over the stencil, which prints on to the paper placed beneath it. As many as 1,000 to 2,000 impressions can it is asserted be thus printed at the rate of four to six per minute. The pen in its essential principle consists of a tube a, fig. 1, tapering to a small point b, and with a needle c moving inside it. This needle is reciprocated with great rapidity, and when the point is projected it is sufficiently long to *reach through* the paper upon which the tube of the pen rests; when

the needle is retracted it is drawn within the tube so that the small end is free to be moved from place to place over the surface of the paper. The needle moving with great rapidity produces the punctures in the paper sufficiently close together to form lines when the pen is manipulated in writing or drawing. The movement of the needle is due to the motion of the armature of an electro-magnet *g*.

This electro-magnet is upon the frame *e*, which supports the axis marked 2 of the fly-wheel *f* (fig. 2), and this fly-wheel is connected with the armature *g*. Upon the axis 2 there is an eccentric or cam with three arms acting upon the stock 3 at the upper end of the needle-bar, so as to give three up and down motions to the needle-point upon each revolution of the axis 2.

The commutator circuit-closer to the electro-magnet, that is to say, "the make and break" arrangement, is composed of the spring *l* acted upon by the notched or flattened disc 4, so as to open and close the circuit through the screw 5, and thus actuate the electro-magnetic motor in the usual manner.

The tube of the pen screws on to the frame *e*, and it is provided with a set-nut by means of which it can be clamped after the tube has been adjusted.

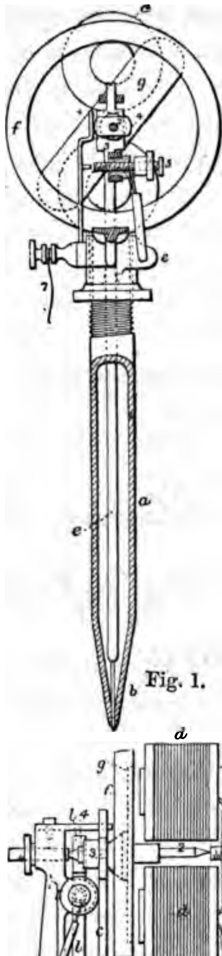


Fig. 1.

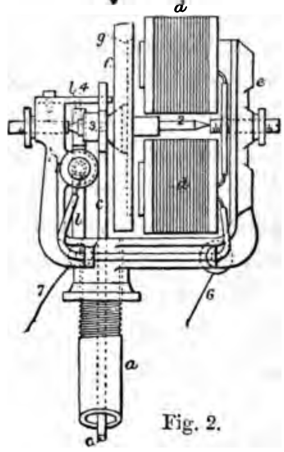


Fig. 2.

The wires 6 and 7 lead to the battery, which consists of two of the ordinary bichromate-cells. The elements are so constructed as to be raised out of action when the battery is not required. The new form of bichromate-battery, introduced by Mr. Fuller, which

dispenses with the necessity for this, is well adapted for working the pen, and is likely to be largely employed for the purpose.

The wires leading from the pen to the battery are flexible to allow the pen to be easily moved about in writing, &c.

The paper to be punctured is placed on a board of white pine, with or without blotting paper. The pen is held and used in the same way as an ordinary pen or pencil. The tube is taken in the hand and pressed on the paper in writing or drawing, and the needle then perforates the paper at the rate of 6,000 strokes per minute. In this way what is being written or drawn is, so to speak, punctured on the paper, as shown in fig. 3. Sometimes

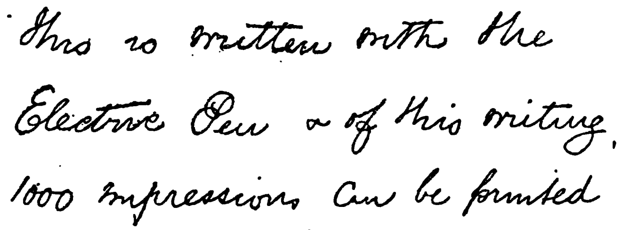


Fig. 3.

prepared paper is used, called "metallic," the tube marking it as a pencil would, or a solid point of metal or black lead is employed; or again, a sheet of carbonized paper is placed under the paper to be pierced, and the needle point pressing on this shows very clearly what is being written or drawn. When the stencil (writing or drawing) is completed it is placed in a frame and a roller coated with prepared ink is passed over it, which fills the perforations. A sheet of paper is then placed under the stencil and the roller is passed over the stencil once or twice and an impression is obtained; this is repeated as required.

The ink used is the best printers' ink, diluted with castor-oil. It is stated that any one can write in their usual style with ease and freedom and with a rapidity equal to that of an ordinary pen or pencil.

An office boy can print the impressions, so the invention will doubtless be of very great service for multiplying copies of letters or drawings.

The price, I may add, for a set of the apparatus complete in every respect is eight guineas.

The Meeting then adjourned

ORIGINAL COMMUNICATIONS.

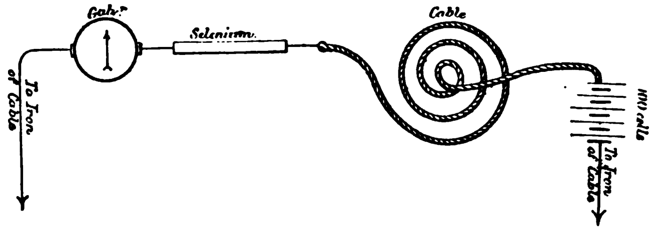
(Read at the Ordinary General Meeting held on March 8th, 1876.)

Gutta-percha Works, 18, Wharf Road, City Road,
March 3, 1876.

To the Secretary of the
Society of Telegraph Engineers.

SIR,—On February 4, 1873, I brought before the notice of the scientific world, through the Society of Telegraph Engineers, the effect which light has on the electrical conductivity of selenium. Since that time the subject has been investigated by several scientific gentlemen, and although nothing new has been brought forward it is satisfactory to find that they all confirm my statement that the phenomena are due solely to the action of light. The subject is now attracting the attention of scientific gentlemen of other countries, and I have had my attention drawn to several inaccuracies which have occurred in reported statements made by those gentlemen. I therefore think it would be well if the Society to whom I first communicated what in time may prove a very important discovery will allow me to place on record a few particulars as regards the actual discovery of the action of light on selenium. While in charge of the electrical department of the laying of the cable from Valentia to Heart's Content in 1866 I introduced a new system by which ship and shore could communicate freely with each other during the laying of the cable without interfering with the necessary electrical tests. To work this system it was necessary that a resistance of about one hundred megohms should be attached to the shore-end of the conductor of the cable. The resistance which I first employed was composed of alternate sheets of tinfoil and gelatine, and, although they answered the purpose, still the resistance was not constant enough to be satisfactory. While searching for a more suitable material the high resistance of selenium was brought to my notice, but at the same time I was informed that it was doubtful whether it would answer my purpose as it was not constant in its resistance. I obtained

several specimens of selenium and instructed Mr. May, my chief assistant at our works at Greenwich, to fit up the system we adopt on shore during the laying of cables, using selenium as the high resistance, and employ the spare members of the staff as though they were on shore duty and report to me on the subject. The arrangement is shown in the accompanying diagram, which is



self-explanatory. It was while these experiments were going on that it was noticed that the deflections varied according to the intensity of light falling on the selenium. One of many of the experiments made was as follows:—

Time.	Selenium closed in box. Gas in room not burning.	Cover off box. Gas not burning.	Cover off box. Two ordinary gas-burners alight in room.
	Resistance in megohms.	Resistance in megohms.	Resistance in megohms.
1′	1483	1419	1047
2′	1483	1405	1018
3′	1483	1405	1018

In each case the temperature in the box was 71·5 Fah. During the laying of the 1873 and 1874 Atlantic cables, the Lisbon and Madeira, Madeira and St. Vincent, St. Vincent and Pernambuco, and the Australian and New Zealand cables, I have with success adopted selenium bars protected from the action of light.

Although I have not been able to devote the time necessary to thoroughly investigate such a phenomena I have taken great interest in the subject, and hope before long to lay before the Society some further experiments which may prove interesting.

Yours very truly,

WILLOUGHBY SMITH.

THOMSON'S SIPHON RECORDER.*

By J. A. Ewing.

§ 1. Preliminary.—The siphon recorder is an instrument for recording on a moving paper ribbon the right and left movements of a pointer acted upon by the successive positive and negative currents which go to make up telegraphic signals. Its extreme sensitiveness makes it especially suitable for recording the signals received through long submarine lines.

A coil of fine insulated wire is delicately suspended between the two poles of a powerful electro-magnet, so as to be capable of moving round a vertical axis, and the current from the cable is made to pass through this coil. When a current is passing through the coil it tends to take up a certain position relatively to the poles of the magnet, namely, the position in which the plane of the coil is perpendicular to the line joining the two poles of the magnet. But the coil is suspended so as to hang (when no current is passing) in a position at right angles to this position. Hence, when a current passes, the coil tends to turn round a vertical axis. If the current is positive, the coil turns round (say) in the direction of the hands of a watch, but if the current is negative, the coil turns round in the reverse direction. Hanging from the coil are two weights which resist the tendency to turn round caused by the passing of a current through the coil, and which serve to bring the coil back to its original position when the current ceases. By means of a system of silk fibres, the motion of the coil is communicated to a pointer which consists of a very fine glass siphon, one end of which dips into an ink holder, whilst the other vibrates across a paper ribbon in obedience to the movements of the suspended coil. The paper ribbon is made

* A considerable part of what follows relating to the Siphon Recorder has been taken, with little alteration, from Mr. J. C. Cuff's pamphlet on that instrument, printed in 1873. The description of the recorder and the directions for its use have been enlarged, and now include all the recent improvements.—J. A. E.

to move past the end of the siphon at a uniform rate. In order to make the ink run through the siphon, the ink is electrified, and the paper ribbon is connected to earth through the framework of the instrument. The ink and paper consequently attract each other, and the ink is spurted out of the end of the siphon on to the paper in a succession of very fine drops. These drops form a continuous straight line along the centre of the paper so long as no current passes through the coil, but when the coil is deflected by the passage of a current, the end of the siphon is deflected with it, and traces a wavy line on the paper, showing the successive deflections to the right or the left of the central position.

The electrification of the ink is effected by means of an electrostatic induction machine called the mouse mill, which is driven either by clockwork or by an electro-magnetic arrangement. The same driving power serves to draw along the paper ribbon past the end of the siphon.

§ 2. GENERAL DESCRIPTION.—Figs. 1 and 2 show the front and side views respectively of a complete siphon recorder, in which the mouse mill is driven by an electro-magnetic arrangement. Figs. 3 and 4 show on a larger scale the suspension of the coil and siphon. A A A are three stout wooden pillars which support the framework of the instrument. B is the mouse mill, which is driven by an electro-magnet inside the box D. E (fig. 1) is one of the terminals of the coil of this magnet, and immediately behind E is the other terminal. The drawer F contains lumps of pumice-stone moistened with sulphuric acid, by which the atmosphere inside the mouse mill is kept dry. The electricity generated by the revolution of the mouse mill is conducted to the brass rod P and is communicated from the point of P to the plate O by convection of the air. The plate O is in connection with the ink holder K, but K is insulated from the rest of the instrument by the vulcanite rod L. The electrification of K causes the ink to flow through the siphon *t* on to the paper which passes along in front of the plate *c*.

The motion of the mouse mill is communicated to the paper drum *d* by means of the hanging shaft J J, which has a large

wooden pulley at one end, and a lead counterpoise with a series of brass pulleys at the other. A cord passes round a pulley at the

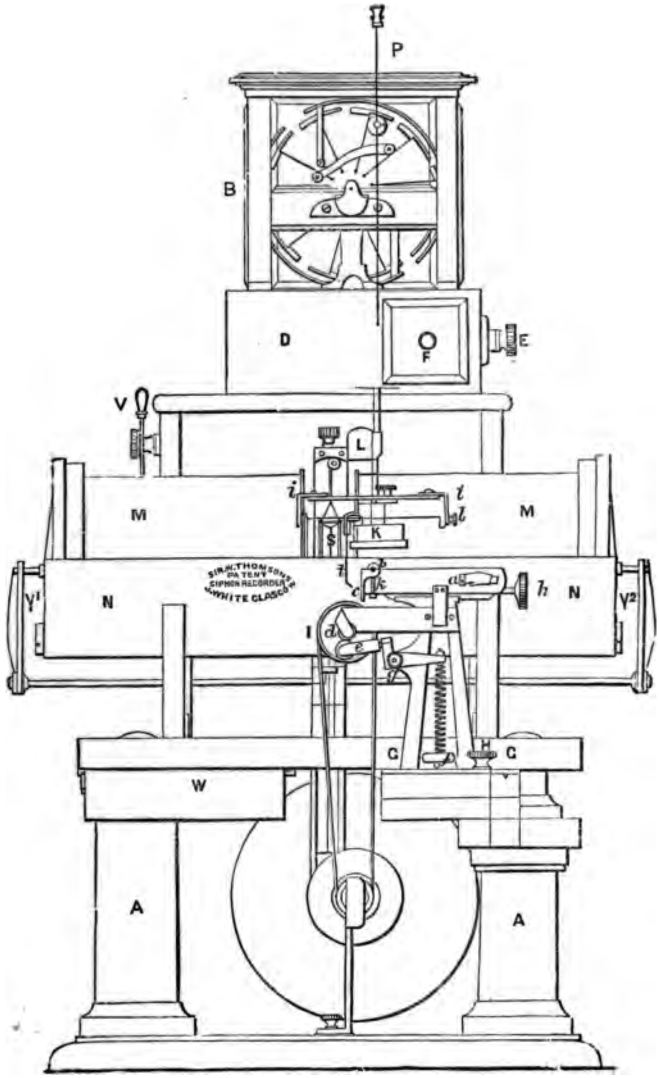


Fig. 1.

back of the mouse mill, and round the wooden pulley at the back of the hanging shaft; another cord passes round one or other of

the brass pulleys at the front of the hanging shaft, and round pulley I, which is on the same axis as the paper drum *d*.

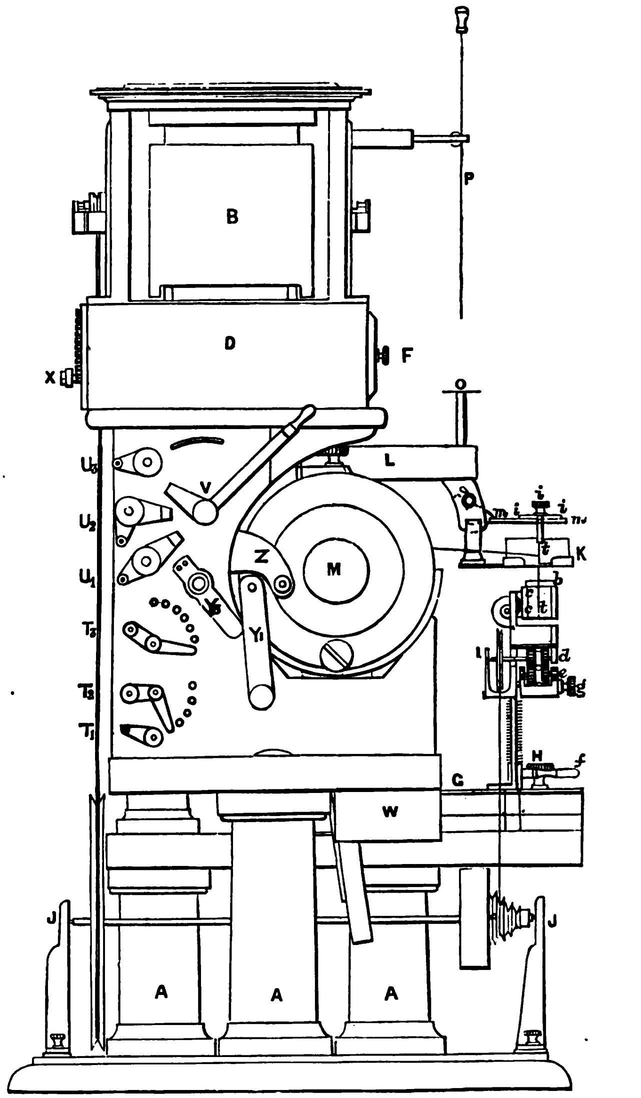


Fig. 2.

M M are the electro-magnets, between the poles of which han the signal coil S. They lie on the semi-cylinders of iron N]

which form at once the bed and armature of the electro-magnets M M. The electrodes of the local battery, which is used to keep the electro-magnets active, are attached to the terminal U_1 and U_3, and a wire from the middle of the battery is attached to U_2. By means of the switch V, full, intermediate, or no battery power may be applied to the electro-magnets M M (see § 7). The contacts with the ends of the coils of the electro-magnets M M are made by the springs Y_1, Y_2, Y_3 (see § 12).

T_1 and T_2 are terminals connected to the ends of the signal coil S. A quadrant slide connected to T_2 enables a shunt to be inserted, so as to lessen the amount of current passing through the coil. W is a small drawer for holding tools, spare siphons, &c.

§ 3. Motion of the Paper Ribbon.—The paper enters from the right-hand side, and is passed under the spring *a* to keep it stretched. Then over the roller *b*, whence it passes over a slightly curved guide-plate *c* vertically downwards past the point of the siphon *t*, till it reaches the driving drum *d*. It passes a quarter round this drum, and is discharged horizontally to the left. It is pressed upwards against the lower edge of the driving drum *d* by the roller *e*. This roller is pressed up against the driving drum by its bearings, these being attached to a brass frame which is pivoted on a stout horizontal pin *g*. A lever projecting to the right is pulled down by a spiral spring, so that the roller *e* may be pressed up against the driving drum, and so may grip the paper. This spring has its lower end drawn down by a crank which is turned by the small handle *f*. The whole stage carrying the paper rollers, &c., is supported on a triangular plate G G, which can be made to move backwards or forwards only, and which is secured in any position by the screw H. The speed of the paper is varied by shifting the cord in front from one to another of the brass pulleys on the shaft J J,—to a larger pulley if the speed is to be increased, or to a smaller pulley if the speed is to be diminished. The ends of the shaft J J press lightly against two vertical guiding cheeks. The cords have to be of such lengths that the ends of the shaft do not rest on the bottom of the guides, and that the wooden pulley does not rub against any part of the framework. Whipcord is the best material for the driving bands. In making them no

splices need be attempted; the reefing knot, with ends not less than one-eighth of an inch long, answers perfectly well, and never

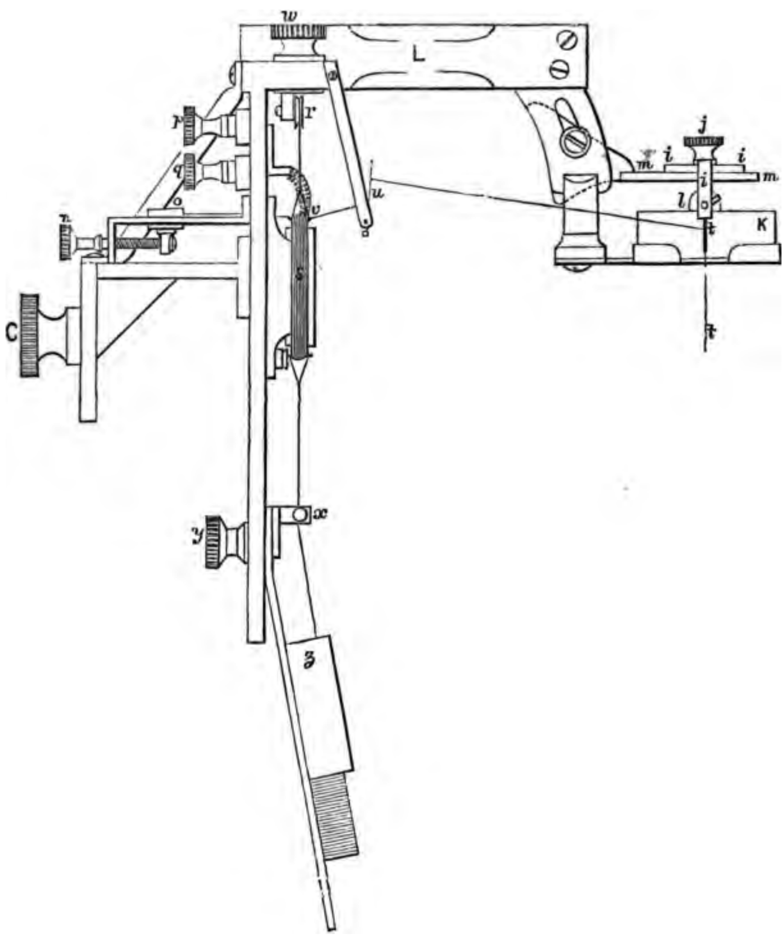


Fig. 3.

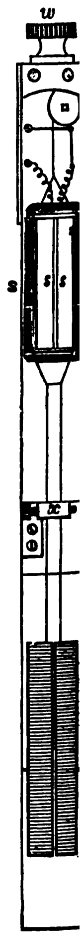


Fig. 4.

draws. Fig. 5 shows the appearance of a reefing knot before it is drawn tight. Care must be taken not to make a "granny's" knot, which is apt to draw. A small variation in the length of either band does no harm.

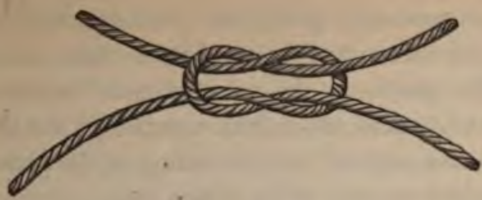


Fig. 5.

§ 4. Suspension of the Signal Coil and Siphon.—The signal coil and the siphon are arranged upon a framework, a side view of which is shown in fig. 3. Fig. 4. shows a front view of the suspension of the coil. The whole framework is secured in its place between the electro-magnets by the clamping screw C at the back. The coil S is suspended by a silk thread passing over the pulley *r*. The position of *r* can be varied by releasing the clamp *w*. Inside the coil is a stationary piece of soft iron *s s*, the object of which is to increase the intensity of the magnetic field in which the coil hangs. Two weights hang from the coil, and can slide up and down the guides *z*. The cords by which these weights hang pass behind a bridge *x*, whose distance from the coil can be altered by releasing the screw *y*. *p* and *q* are the terminals of the coil to which wires inside the instrument leading to T_1 and T_2 are fastened. At the right-hand top corner of the coil a fine silk fibre *v* is attached, which leads to a point near the end of a small vertical lever *u* called the multiplier. Near the top of *u* another fibre is attached, which leads to *t*, a projecting point on the left-hand side of an aluminium cradle which carries the siphon. This aluminium cradle is fixed to a cross wire on the bridge *i i*; by turning the screw *l* (fig. 1) torsion can be put on this wire. At the back of the signal coil, and just behind the point of attachment of the fibre *v*, is another fibre, the other end of which is attached to a spring at *o* (fig. 3), the position of which can be altered by turning the screw *n*. By turning *n* outwards a greater strain is put on the fibre leading from *o* to the signal coil, and by turning *n* inwards the strain is lessened. The bridge *i i* which carries the siphon is secured to the plate *m m* by the screw *j*, and is capable

of being moved backwards or forwards when *j* is loosened. By pressing the plate *m m* up gently, the plate, and with it the bridge carrying the siphon, can be lifted so that the siphon rises out of the inkholder. The groove in the piece of metal between L and *m* constrains the plate *m* to rise in such a path that the fibres leading to the coil are not strained by the raising of the plate. K is the ink box into which one end of the siphon dips.

§ 5. THE MOUSE MILL.—The mouse mill is at once an electro-magnetic engine and an electrostatic induction machine. The electro-magnetic arrangements are as follows:—In the box D is a horse-shoe electro-magnet. In the glass case above are ten revolving armatures, which pass the poles of the magnet at the lowest point of their revolution. On the shaft which carries the armatures, and revolving with it, is a cam, which is a ten-sided polygon. (This cam is at the back of the mill, and is not shown in the figure.) On the edge of this cam a contact-spring rests, and as the corners of the cam pass under the contact-spring they raise it, and so break contact between it and a stop underneath it. When the middle portion of the straight edges of the cam passes, the spring drops and contact is made. This contact determines the passage of the current from a powerful local battery through the coils of the electro-magnet in the box D. The cam is so set that, when the mill revolves, each successive armature is attracted by the electro-magnet so long as the armature is approaching the magnet, but when the armature passes the poles of the magnet contact is broken and the attraction ceases. This makes the axle carrying the armatures revolve. To diminish friction as much as possible the ends of the axle are pivoted on the edges of friction wheels, which work in cups filled with oil. By means of the quadrant slide X (fig. 2) at the back, extra resistance can be introduced into the circuit of the electro-magnet; this lessens the strength of the current passing through the coils of the magnet, and thus reduces the speed of the mill. The lowest stud of the slide corresponds to the highest speed.

The electrostatic arrangements in the mouse mill will be understood by reference to fig. 6. On the revolving axle are fixed ten metal carriers (c, c', &c.), insulated from the axle and from each

other. They revolve inside two metal plates, I and I′, bent so as to form parts of cylinders (called the inductors), one of which, I′, is in contact with the framework of the instrument, and so to earth; the other, I, is insulated and connected to the rod P (fig. 2). Attached to the carriers and in electrical contact with them are ten pins, which, as they revolve, touch successively the four contact springs *a*, *b*, *a′* and *b′*. The inductors and the springs are fixed; the carriers and the contact pins revolve. The spring *a* is connected to the inductor I, *a′* is connected to I′; *b* and *b′* are connected together, but are insulated from the framework.

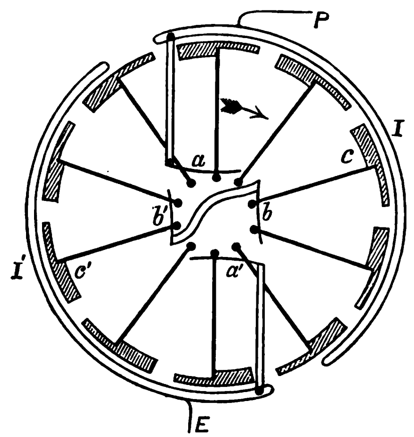


Fig. 6.

Suppose that the inductor I is in a state of very feeble (say) positive charge to begin with, and that the carriers are set revolving. The two opposite carriers *c* and *c′* are in contact with each other while the pins connected to them are passing the springs *b* and *b′*. During this time, the positive charge on I induces a separation of electricities on *c* and *c′*, attracting a negative charge to *c*, and driving off positive electricity to *c′*. As *c* and *c′* revolve, *c* comes in contact with *a′*, and its negative charge flows to earth; *c′* comes in contact with *a*, and its positive charge goes to increase the previously exising positive charge on the inductor I. This has the effect of increasing the inductive action of I on the succeeding carriers. Thus not only does the charge on I go on

increasing, but the rate of its increase goes on increasing too. The action will take place if the potential of the inductor I differs ever so little from that of the inductor I′. If I were negative relatively to I′ to begin with, then a negative charge would accumulate on I. It would be difficult, if not impossible, to reduce I and I′ so exactly to the same potential as to prevent I from getting highly charged after a few turns of the mill.

The carriers and inductors are coated with paraffin wax to prevent sparks from passing across the air space between them.

§ 6. THE LOCAL BATTERIES.—The batteries employed to keep the electro-magnets M M and that in the mouse mill active are modifications of Daniell's battery, designed so as to have very little internal resistance, and are called *Tray* cells. They consist of large flat wooden trays, lined with lead to make them watertight, and having for the positive metal a sheet of thin copper in the bottom of the tray. In the four corners of the tray four stoneware props are placed, and on the top of them rests a zinc grating which forms the negative metal. The tray is filled up with a solution of sulphate of zinc, and sulphate of copper crystals are dropped in on the bottom plate. The zinc is surrounded with a sheet of stout parchment paper to prevent the diffusion of the sulphate of copper solution from producing copper deposits on the zinc plate. The trays are connected to each other by being piled one on the top of another, so that the copper of one is connected to the zinc of the one below it, by contact of the lead sheathing of the upper one with the four corners of the zinc plate below on which it rests. The advantage of this form of cell is its extremely low internal resistance. It requires constant attention to keep it in efficient order. (See § 8.)

§ 7. DIRECTIONS FOR SETTING UP THE BATTERIES.—Each of the lead trays must first be carefully coated with varnish, over the bottom, sides, and edges. Spirit varnish made with shellac or ordinary varnish will do very well. Care should be taken not to varnish over the strip of copper which is soldered to the bottom of each tray. The under surface of each thin copper plate is also to be varnished. When the varnish is dry, place the sheet of copper in the tray with the varnished side down, and, cutting a

slit in the centre of the copper sheet, bring through the strip of copper which is soldered to the bottom of the tray; bend the strip and spring it so that its end presses firmly against the upper surface of the copper plate, taking care that both are scraped clean at the place where they touch.

Each lead tray should have a stout copper wire soldered to it, projecting about three inches from one corner.

Each zinc is to be protected by a square of parchment paper bent round below it, and folded neatly at the corners and fixed with sealing wax.* Care must be taken that the edge of the paper be generally $\frac{3}{4}$ inch (and in no place less than $\frac{1}{4}$ inch) above the upper level of the bars of the zinc grating. It must be bound firmly to the zinc by twine passing under the parchment paper and tied over the zinc above; also by a long piece of twine several times round the square.

To support a pile of trays, take four blocks of wood or stone, each four or five inches square in horizontal dimensions and of any convenient height, and place them on the floor in positions to bear the four corners of a tray. The pile must be so placed as to give ready access to each of its sides. Put a piece of thick sheet gutta-percha, six inches square, on the top of each of the wooden squares, and then lay down the first tray upon them, seeing that it is properly levelled. This is most easily done by pouring a small quantity of water into the cell, and seeing whether it lies evenly over the bottom. Put four stoneware blocks, each about $1\frac{1}{2}$ inch cube, in the corners of the tray, on the top of the copper sheet, and put one of the zinc gratings resting with its four corners on these props. Put a solution of sulphate of zinc of specific gravity about 1·1 into the tray,† pouring in first between the

* Press the paper against the zinc between finger and thumb on each side of the corner, and draw the bight or bend of the paper diagonally away from the corner; then fold the bight round the vertical corner of the zinc, and press it against the flat zinc surface on one side or other of the corner. Secure with sealing wax in the bight, and where one side of it is pressed against the paper on the vertical zinc surface. Then tie the paper round carefully with cord in the manner described in the text.

† This solution may be prepared by mixing 1 part by weight of the salt to 5 parts of water, or 2 lb. of salt to 1 gallon of water.

edge of the tray and the parchment paper, and afterwards filling up to the level of the top of the zinc grating by pouring some of the solution directly on the zinc over the paper. See that the top corners of the zinc, and the bottom corners of the tray to rest upon it, are all properly tinned, scraped clean, and dry. Place a lead tray resting with its four corners on the upper projecting corners of the zinc. Place four stoneware props in the corners of this second tray, put a second zinc upon them, and fill with solution as before. Proceed thus until a pile of from six to ten trays, one over the other, is made and filled with liquid. Solder a stout copper wire to one of the corners of the top zinc, to serve as an electrode. In the same way make as many piles as are required. Leave a space at least one foot broad between each pile and its neighbour. Connect the piles in series, the top zinc of one pile to the lowest lead tray of the next one.

The crystals of sulphate of copper to be used should be broken into small pieces, and weighed out in quantities of an ounce each. To put the battery in action drop in four ounces to each cell; one ounce separetely on each side, distributing it as equally as may be along the space between the stoneware props. In putting in the crystals be careful not to let any fall inside the parchment paper, or in contact with the zinc. As soon as the sulphate of copper is put in, the battery should be allowed to work, either on short circuit or on the circuit which it is intended for.

From three to six cells are required to drive the mouse mill. The number to be applied to the magnets M M varies with the circumstances of the case; it may be anything from one to twenty, or even more. Separate sets of cells should be used for the magnets and for the mill. If the same set is used for both,—that is to say, if the coils of the magnets are to be looked on as a *shunt* applied to the battery which is driving the mill (or *vice versa*), then the whole number of cells in use should be applied to each circuit. The practice of using part of the battery employed to work the magnets to work the mill also is very objectionable. A wire from the zinc pole of the pile or piles intended for the magnets comes to the terminal U_1 (fig. 2) from the copper pole to U_3, and a wire from an intermediate tray in the series to U_2.

This gives the means of applying full or intermediate battery power to the magnets by means of the switch V.

§ 8. Maintenance of the Batteries.—When the tray cells are in use the sulphate of copper is decomposed, copper is deposited on the copper plate, and the zinc plate is consumed; sulphate of zinc is formed, which strengthens the solution at the top of the cells; it is therefore necessary to supply more crystals of sulphate of copper, and also to draw off the sulphate of zinc solution from the top of the cell when it becomes too dense, and to supply its place with fresh water. When a cell is in constant use it is desirable to draw off a little of the liquid daily. The drawing off is effected by means of a siphon, the shorter end of which is dipped into the cell between the edge of the tray and the zinc plate, so as to be just below the lowest level of the zinc, while the longer end stands out over a convenient vessel to receive the liquid. Water is to be poured into the space above the zinc grating by means of a funnel ending in a bent tube. The specific gravity should be frequently tested by a hydrometer or by specific gravity beads, and the quantity drawn off should be regulated so as to keep the specific gravity of the liquid (taken from near the surface of the cell) at about 1·24, or not greater than 1·3 and not less than 1·12. Fresh crystals of sulphate of copper are to be dropped in along the four sides of the cell, one ounce at a time along each side. It is easy to see when fresh sulphate is required by observing when the blue colour of the liquid at the bottom of the cell disappears. When cells are in active use they generally require new sulphate almost daily. A cell, or pile of cells, should never be left out of use for any length of time with crystals or blue solution in it; before being left it should always be short-circuited until the liquid becomes clear and colourless. If sufficient care is not taken to remove the sulphate of zinc solution as it becomes too dense, crystals of sulphate of zinc will accumulate round the edges of the cell. These ought to be scraped off. When the batteries are not properly attended to, these crystals will often accumulate in such quantities as to connect the zinc tray to the lead casing, and so to short-circuit the cell. The battery should be frequently tested in the manner described farther on (§§ 15, 16, and 17).

§ 9. Adjustment of the Mouse Mill.—*If the Mill does not run fast enough.*—1st, Alter the quadrant slide X (fig. 2) if not already on the stud of highest speed; 2nd, Look well to the adjustment of the contact-breaking spring at the back; 3rd, See that the cups are well supplied with oil in which the friction rollers work that bear the ends of the main shaft; 4th, If it still goes too slowly more battery power must be employed.

The electro-magnetic contact breaker at the back consists of two platinum points, one of which is fixed, whilst the other is carried up and down by a steel spring which is raised at intervals by the cam. If these platinum points are separated too much, the electro-magnet will not act on each carrier in succession so long a time as it ought, and diminished efficiency is the result. Again, if the points remain in contact too long, the electro-magnet will continue to act on each carrier after it has passed its poles, and thus tend powerfully to retard its progress. Hence the adjustment of this spring, by turning the screw which raises or depresses the lower contact, is most important; but when once set right it will remain so for a long time.

If the ink is insufficiently electrified when the mill is running properly, and generating electricity—1st, Alter the distance of the rod P from the plate O. Generally about two or three inches is found to be the best distance; but this depends on the state of the atmosphere, &c.; 2nd, See that the insulation is nowhere impaired by dust. If the silk fibre attached to the siphon has any dust upon it, a camel's-hair brush or feather will remove it. The vulcanite rod L, in particular, must be kept free from dust, and also the metal work near it. It is sometimes necessary to clean the vulcanite rod L by washing it with warm water and a little soap, and then drying it carefully.* The paraffin tube, which insulates the rod leading from the insulated inductor to the rod P, will also require occasional cleaning.

When the mouse mill fails to generate electricity, although otherwise running well, it will be necessary—1st, To take off the top

* When other means of getting L to insulate properly fail, it should be painted with varnish, prepared by dissolving sealing in warm spirit. This varnish should be laid on while hot.

and carefully remove the insulated inductor, and see that its insulation and that of the carriers is perfect; 2nd, Make sure that the four springs make contact properly with the brass pins as they come round in succession. Every provision is made for the adjustment of these springs, or for the insertion of new ones, which is easily effected. Several sets of spare electrostatic springs, and one spare electro-magnetic contact-spring for the driving circuit are sent with each instrument; 3rd, Supply the drawer F with sulphuric acid by pouring a few drops on each piece of pumice. The strongest commercial sulphuric acid should be employed for this purpose, and should be prepared by boiling it for half an hour in a Florence flask along with a little sand to facilitate ebullition, and a few crystals of sulphate of ammonia. The flask should be supported by a retort stand over a spirit lamp or other convenient source of heat, beneath which is placed a pan of cold ashes or other arrangement to prevent any injury being done in case the flask breaks.*

If too much electricity is generated, the siphon will not mark well, and will sometimes vibrate laterally unless some is drawn off by a pointer in connection with the outside of the case and directed towards the rod P. If the instrument is not provided with such a pointer the want can be easily supplied by attaching a piece of wire about 6 inches long to the screw handle of the drawer F. The lateral vibration of the siphon may be further prevented by enclosing the paper (while running) in a box containing aqueous vapour, or (when necessary) by using prepared paper. The paper may be prepared by soaking it in a solution of two parts of nitrate of ammonia in 100 parts of water, and then drying and re-rolling it. This salt, being a deliquescent, keeps the paper slightly damp by absorbing moisture from the atmosphere, and by thus increasing the conducting power of the paper, prevents the lateral vibrations of the siphon.

If sparks are seen passing between the inductors and carriers at

* A good plan of keeping the mouse mill dry, in a cold and damp climate, is to take out the drawer F, and put in its place a coil, or bend of lead tube, through which a current of hot water or steam is kept passing from a small tin boiler, kept hot by a gas jet or by the flame of a candle.

any place, they will be due to a defect in the coating of paraffin wax. Such defect can be easily repaired by a little hot melted paraffin laid on with a brush.

If sparks are seen at the contact of the revolving cam at the back and the spring that rests on it, they will be due to a defect of insulation of the battery wire or electro-magnet coil, which ought to be remedied without delay.

§ 10. Adjustment of the Paper.—*To release the paper,* turn the handle *f* in a direction opposite to the motion of the hands of a watch. Thus the roller *e* is allowed to fall about an eighth of an inch, and the paper is quite free, so that it may be slipped out with ease, and, though the driving drum *d* revolves, the paper is not drawn along. *To clutch the paper against the driving drum,* turn the handle *f* in the reverse direction.

To regulate the distance of the paper from the point of the siphon, turn the milled head *h*.

To bring the line made by the siphon to the middle of the paper, loosen the clamping-screw H, when the whole paper stage G G can be easily moved backwards or forwards to the desired position.

To make the paper run evenly between the rollers d and *e*, turn the small screw *k*, which will either elevate or depress the nearer end of the roller *b* until all works true.

To alter the speed of the paper, shift the band in front from one pulley to another. If necessary alter the speed of the mill.

§ 11. Adjustment of the Siphon and Signal Coil.—A large number of fine glass tubes for siphons is provided with each instrument.* The siphons are made from them as follows:—Take one of the small pieces of tube and present it to the heated atmosphere surrounding any small flame, such as that of a match, in a convenient position to allow one part to drop by its own

* Should these prepared tubes not be at hand, they can be easily made thus:—Take a piece of soft glass tube, about one quarter of an inch in diameter, the thickness of its wall being about one-sixth of the whole diameter. Soften about one inch near the middle in an ordinary gas flame, or, better still, over a Bunsen burner, slowly turning it round the whole time. When sufficiently softened, remove it from the flame and pull the ends apart until the tube is so drawn out that it is of the desired diameter. Break the fine tube thus produced into pieces about four inches long.

weight when the tube softens. The tube should be brought into contact with the lower, not the upper, edge of the flame. It can thus be bent into the proper shape, the long limb being about two and a half inches in length. The point should be bent so as to make an angle of about 130° with the longer limb. Figure 7 shows a properly made siphon, drawn to full size. In making the siphon care must be taken that the bends are not over-heated, so as to cause the tube to collapse and diminish its bore. Any narrowness in the bore at the bends is easily detected by filling the siphon with ink. When an ordinarily fine siphon is used, the ends may be broken by the fingers or otherwise, so as to be of the proper length. On short circuits, where great sensitiveness is not required, a thick siphon may be used, and the ink can be made to run well without electrification. In this case the point of the siphon should be nicked with a glass-cutter's knife, when it will break off flush. It may then be ground parallel to the paper on a small corundum grindstone, and welted by being held for an instant in a flame, so as to produce a perfectly smooth point.

Fig. 7.

The siphon is readily secured in position, on the aluminium cradle which carries it, by a little beeswax.

To attach or remove a siphon, raise the piece *m m* (fig. 3) which carries the siphon bridge *i i*, and is so guided by the curved V groove above as not to disturb the signal coil. When the siphon is clear of the ink bottle, apply a hot wire to the back of the aluminium cradle; this will melt the wax, and the new siphon can at once be stuck on or the old one removed.

To adjust the siphon relatively to the signal coil.—1st. The signal coil must hang freely and evenly about the soft iron magnetic inductor *s s* (fig. 4), and must not touch it at any point.

2nd. All the fibres must be sufficiently strained.

3rd. The normal position of the siphon must be vertical.

To effect the first of these adjustments the coil is raised or

lowered by turning the screw *r* with a square-pointed key. It is moved backwards, forwards to the right, or to the left, by easing the screw *w*, shifting as desired, and then reclamping.

To attain the second of the above conditions in connection with the first, the three screws *n*, *j*, and *l* (figs. 1 and 3), must be manipulated.

By loosening *j* and sliding the bridge *i i* backwards or forwards, the first approximation is obtained. Then the screws *n* and *l* must each be turned so that the steel spring connected with *n*, and the torsional elasticity of the wire attached to *l*, may re-act on each other, so as to make the signal coil hang in a line with the poles of the magnet.

Lastly, to make the siphon ha g vertically, the screw *j* will probably require to be undone, the bridge *i i* slightly shifted, and another touch given to *l*. This last adjustment is the only one required in general use.

See that the siphon does not stick on account of the shorter end being too long, and touching the bottom of the ink box.

The most suitable ink for the recorder is the best soluble aniline blue. Put as much of the crystals as will stand on the small blade of a pen knife into a 3 or 4 oz. bottle of water, and shake them up, and you will immediately have a perfectly fluid ink of a deep blue colour. This ink is superior to any ordinary kind, because it does not thicken or precipitate, and, in the form of crystals, is far more portable.

§ 12. General Directions.—The size of the signals may be varied by means of the shunt attached to T_2 (fig. 2).

The sensitiveness of the instrument may be increased in several ways:—

1st. By altering the contact-springs of the electro-magnets. When the contact-springs are arranged as shown in figs. 1 and 2, the magnet coils are connected up in series. If the spring Y_1 be shifted over to the stud Z, the spring Y_3 remaining disconnected, the right hand coil only is in circuit. This gives a low degree of sensibility. But if, when Y_1 is connected to Z, the spring Y_3 be put upon the stud to the left on which Y_1 is shown as resting in fig. 2, then both coils are in circuit, and are joined up in multiple

arc. Except with a battery of exceptionally high resistance, the last arrangement is the most sensitive of all three. Each of the coils has a resistance of about 8 ohms; consequently, when the first arrangement is adopted, the total resistance is 16 ohms; with the second arrangement 8 ohms; and with the third arrangement 4 ohms.

2nd. The sensibility of the instrument may be increased by lowering the brige x, fig. 3 (by loosening the clamp y), so as to lengthen the distance between it and the signal coil.

3rd. By using light weights. The only limit to this is that the weights must be sufficiently heavy to bring the coil (after being deflected) back to its normal position so quickly as to leave no sensible interval between the cessation of the current and the corresponding return of the coil. Since the first introduction of the recorder it has been found that the weights may be much lightened. Weights of seven-eighths of an ounce each are found to give sufficient directive force.

4th. The sensibility may be greatly increased by bringing the two cords by which the weights hang from the coil close together. The closer these threads are to one another the less will be the directive force on the coil, and the greater will be the sensibility. A good plan of bringing them very close together is to tie a thread round them just below their points of attachment to the coil. They may thus be brought so near each other as to touch. They should then be brought equally close to one another where they press on the bridge x, so that they may hang parallel between the coil and the bridge. This mode of increasing the sensibility of the instrument is recommended in preference to the 3rd method given above, because when the weights are much lightened the stability of the coil is lessened, and it is liable to be affected by any unsteadiness of the instrument or of the table; while by bringing together the suspended cords of the weights the directive force may be reduced to any extent without taking away from the stability of the coil.

5th. The leverage of the multiplier u can be increased by lowering the point of attachment of the fibre leading to the coil, or by raising the point of attachment of the fibre leading to the siphon.

On short circuits the multiplier *u* can be dispensed with, and the coil connected directly to the siphon by a single fibre. The multiplier is removed by taking out the two screws (shown in fig. 4, just below *w*) which secure the frame which carries the multiplier.

When the instrument is to be out of use for (say) some hours, the battery should be disconnected from the electro-magnets by turning the switch V. The mouse mill should be stopped by turning X (fig. 2) down so as not to touch any of the studs. The siphon should be lifted out of the ink-bottle by raising *m m*, and the ink should be sucked out of the siphon so as to prevent it from drying in the siphon and causing it to clog. When the siphon is being sucked care must be taken not to strain the fibres.

When the instrument is to be out of use for a shorter time, the cord which drives the paper drum may be placed on the pulley giving the lowest speed, so as to waste as little of the paper as possible; or the paper may be entirely stopped by turning the handle *f*, when the ink from the siphon will accumulate on it in a large drop. The mill should never be stopped without the siphon being raised and sucked.

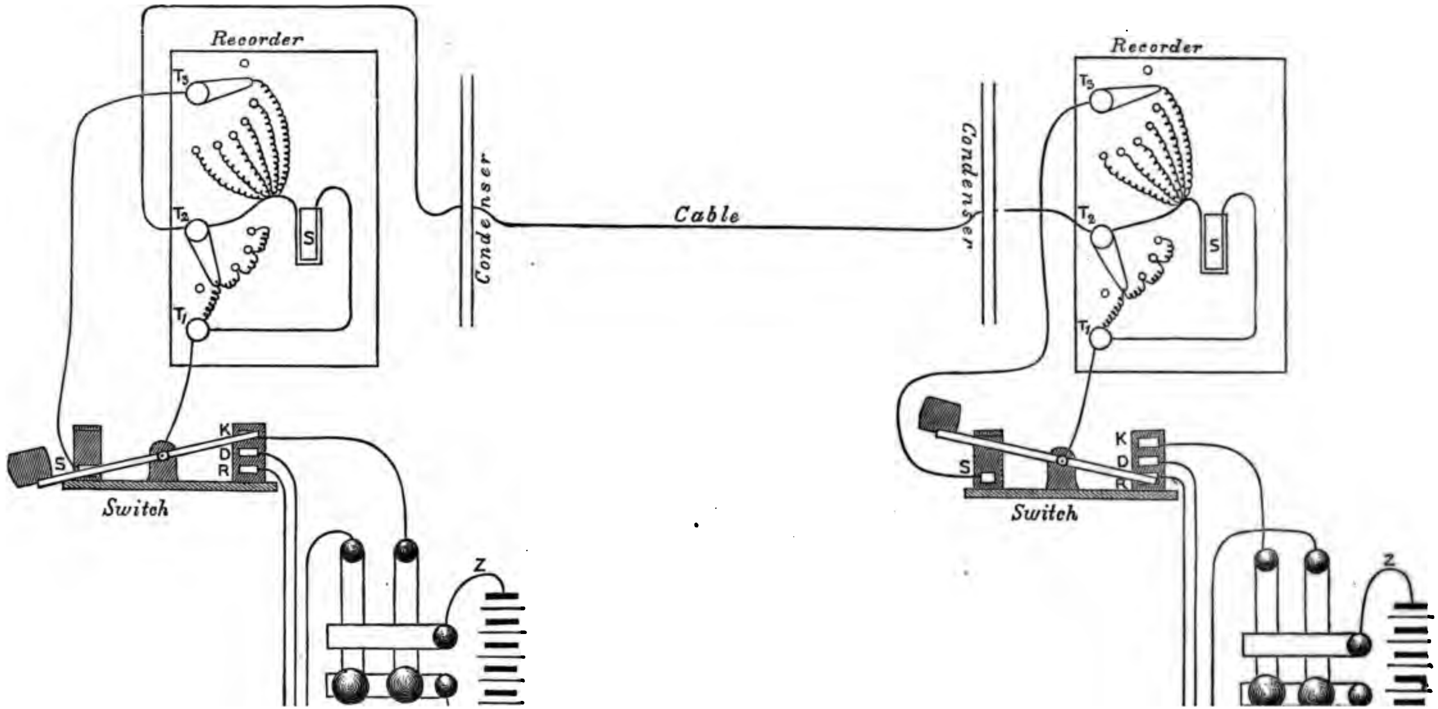
When the instrument room is on the ground-floor, a brick or stone pillar should be built for the recorder to stand upon. The top of this should be about nine inches below the level of the table. The upper part of the recorder then projects through a large square hole in the table. This arrangement prevents the recorder from being shaken by any movements of the table, and it brings the end of the siphon down to a convenient level. When a pillar rising from the ground cannot be used, the recorder should stand on a stout bracket projecting from a wall (not a partition) at the same distance below the level of the table. The instrument should be placed so as to be well lighted from the left.

§ 13. Connections for Sending and Receiving.—In order that the signals sent from a station may be recorded on the receiving instrument at that station, it is necessary to send a portion of the current through the coil of the recorder at the sending station. The portion of the current which passes through the coil of the recorder at the sending station may either rejoin the re-

mainder of the current before it enters the cable, or may be allowed to go to earth at the sending station. These two possible systems allow of the adoption of either of two arrangements of the connections. There are several plans by which each of these two systems may be carried out. Fig. 8 shows an arrangement of the connections in which no portion of the sending current goes to earth at the sending station.

T_1, T_2, and T_3, are the three terminals on the left hand side of the recorder. On the uppermost of these (T_3) there is a quadrant slide, by means of which small resistances, varying from 8 ohms downwards, can be inserted between T_3 and T_2. T_2 and T_1 are connected to the two ends of the signal coil S. Attached to T_2 is another quadrant slide, by means of which resistances, varying from 500 ohms upwards, can be inserted between T_2 and T_1, so as to form a shunt to the coil. When the sliding piece connected to T_2 is not in contact with any metallic stud, the only connection between T_2 and T_1 is that given by the coil itself—in other words, the coil is not shunted at all. T_2 is connected to the cable or to the condensers, if any are used. The current from the key enters the recorder at the sending station at T_1 and T_3 simultaneously. By far the greater part reaches T_2 by way of T_3, through the low resistance in the upper quadrant slide. A small portion reaches T_2 from T_1, passing partly through the signal coil, and partly through the shunt between T_1 and T_2, should there be one. The joint resistance between T_1 and T_2 of the coil and shunt is very much greater than the resistance between T_3 and T_2, and hence only a very small portion of the current goes to T_2 by way of T_1. Hence only a very small portion of the current passes through the coil.

The switch shown in fig. 1 consists of a metallic lever moving about a horizontal axis. Its centre is connected to T_1. When the switch is set for sending, K is connected to S, which is connected to T_3; therefore the key is connected both to T_1 and T_3, as described above. To set the switch for receiving, the handle is raised. T_1 is thus connected to earth, and S is left insulated. Then the current from the distant station entering the recorder at T_2 can find no outlet by way of T_3, but must pass from T_2 to T_1



Recorder
T_3
T_2
T_1
S
Condenser
Cable
Condenser
Recorder
T_3
T_2
T_1
S
K
D
R
S
Switch
Z
K
D
R
S
Switch
Z

through the coil and shunt. The contact-piece D allows the cable to be discharged to earth during the movement of the switch from "send" to "receive." The lever ought to make contact with D before it breaks contact with S, so that the cable may be for an instant connected to earth through no other resistance than the very small one between T_2 and T_3.

It will be seen that the batteries are reversed at the two stations, or, what amounts to the same thing, the positions of earth and line are different on the two keys. This is necessary in order that the sending and receiving signals may be recorded in the same direction, when the arrangement shown in fig. 8 is used.

§ 14. SECOND METHOD OF ARRANGING CONNECTIONS.—The method shown in fig. 8 wastes none of the current at the sending station. The portion of the current which passes through the coil at the sending station rejoins the main body at T_2. Fig. 9 shows an arrangement in which the portion of the current which records the signals at the sending station is allowed to go to earth. At the sending end the contacts numbered 2 and 3 are made, and No. 1 is broken. At the receiving end No. 1 is made and Nos. 2 and 3 broken. The current from the key enters the recorder simultaneously at T_1 and T_3, which are therefore at the same potential. Between T_2 and the earth there is a high resistance—say of 5,000 ohms—which, however, may be varied to suit different cases. Since the very low resistance between T_3 and T_2 is an exceedingly small fraction of the high resistance that there is between T_2 and earth, T_2 will be at a potential very little lower than that of T_3. But T_1 is at the same potential as T_3, consequently T_2 is at a potential very little lower than that of T_1. There will therefore be a feeble current from T_1 to T_2 through the signal coil.

To receive, the contacts Nos. 2 and 3 are broken, and No. 1 is made. T_2 is thus put in direct connection with the earth, and the received current enters at T_1, passes through the coil, and goes to earth at T_2. In this arrangement the received and sent currents, when of the same name, pass through the signal coil in the same direction. There is, therefore, no need to reverse the batteries at the two ends as in the former arrangement. The switch must be

so designed that when it is turned over to "send," contact No is first made, then No. 1 broken, and lastly, No. 3 made; a when turned over to "receive," No. 3 must first be broken, th No. 1 made, and lastly No. 2 broken. This order preserves t coil from receiving any violent shock by the discharge of t cable.

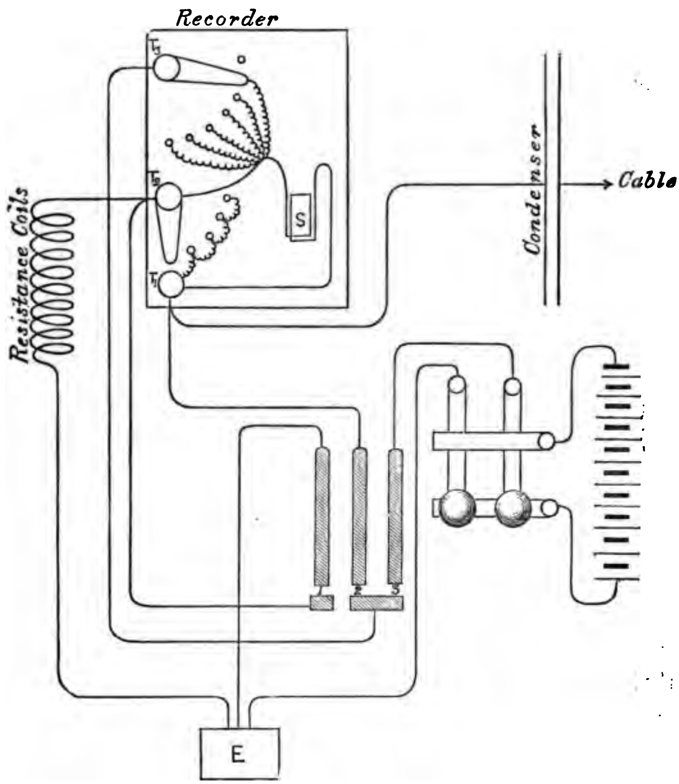


Fig. 9.

The sending signals recorded by means of the second of t above arrangements are very much more legible than those o tained by the first. The latter are much more abrupt and jerk This is especially the case when condensers are used, or when t automatic transmitter is employed (see § 40).

§ 15. Tests of the Batteries.—The tray cells ought to be tested frequently, in order that any undue rise in internal resistance or defect in electromotive force in any one of them may be detected and remedied. The whole pile of cells in circuit should first be tested, both for resistance and electromotive force, and then each single cell should be tested. If the resistance of any single cell is much greater than the total resistance of the pile divided by the number of cells in the pile, or if the electromotive force of any single cell is much less than the total electromotive force divided by the number of cells, then the defective cell should be at once short circuited or removed, because its presence in the circuit interferes with the efficiency of the battery.

The tests of the battery may be very conveniently made by means of a mirror galvanometer, or of a common tangent galvanometer, or of a quadrant electrometer. The wires used to lead from the battery to the testing table should be thick, so as to have no sensible resistance. It is convenient to have a box of resistance coils, the amount of which can be easily varied. A coil of stout cotton or silk-covered copper wire, having a *known* resistance of not less than one-tenth ohm, and not more than one ohm, should also be prepared; this should be made of thick wire so as not to get much heated by the passage of a current during the test. This coil (which for brevity will be called the "battery shunt") should be provided with thick flexible electrodes, so as to be suitable for direct application to each cell.

§ 16. To find the Resistance of a Cell or Pile of Cells.—Make a circuit consisting of the cell, the (tangent or mirror) galvanometer, and a high resistance,—such that the resistance of the cell itself is insignificant compared with the resistance in the circuit external to the cell. If the galvanometer has a high resistance, say of at least 3,000 ohms, it is unnecessary to insert any additional resistance, for the resistance of the galvanometer alone will then be such that the circuit will fulfil the condition of having the resistance external to the cell immensely greater than the resistance in the cell itself. Observe the deflection given by the galvanometer, and let it be called D. Next shunt the cell, by connecting directly to its two terminals the terminals of the battery

shunt coil mentioned in the preceding paragraph, and allow the rest of the circuit to remain unchanged. Observe the deflection now obtained, and call it d. If we call the resistance of the battery shunt S, then R, the resistance of the cell, will be found by the following formula—

$$R = S \frac{D - d}{d}.$$

This formula holds good only when the resistance external to the cell is very great compared with that of the cell itself. It is therefore necessary, when the external resistance is that of the galvanometer alone, that the galvanometer should not be shunted—the deflection may be brought within reasonable limits by applying strong directing magnets. If, however, a sufficient extra resistance be put into the circuit, there is no objection to the use of a shunt on the galvanometer. It should, however, be borne in mind that the above formula is never more than approximately true, and that it is more and more nearly true the nearer the ratio of the external to the internal resistance approaches to infinity. Hence the higher the external resistance is the more near will the result given by this formula be to the truth. This test is not applicable to a battery of high resistance, but it is by far the best test for tray cells, or even for some of the more ordinary forms of batteries. When a quadrant electrometer can be used, the test is exceedingly simple. Observe D, the deflection obtained when the two poles of the battery are connected to the two electrodes of the electrometer. Next observe d, the deflection obtained by connecting the poles of the battery to the electrometer, the battery being shunted through the coil whose resistance is S. Then as before—

$$R = S \frac{D - d}{d}.$$

When the electrometer is used this formula is rigidly accurate,* and the test is applicable to a battery of any resistance whatever.

A convenient modification of this test is to use a box of adjustable resistance coils as the battery shunt, and so alter S until the

* Except for a slight change in the electromotive force of a cell during the test, due to polarization.

second deflection d is exactly half the first deflection D. Then R will be equal to S.

In observing d care must be taken not to allow the current to run through the shunt coil for any considerable length of time, as the coil becomes rapidly heated and so alters in resistance. In order that the rate of heating should be as small as possible, the shunt coil should be made of thick wire. If the deflections D and d are so nearly equal that the difference between them is small compared with either, then the resistance of the shunt is too great, and a shunt of less resistance ought to be used.

The resistance of the tray cells depends, of course, upon their size. They have seldom less internal resistance than 0·1 ohm, and should never have more than 0·5 ohm. It is very important that all the cells in a circuit should have approximately the same resistance.

§ 17. To Test the Electromotive Force of the Battery.—The electromotive force of each of the tray cells should be tested occasionally by comparing it with the electromotive force of a standard cell of a constant kind. The standard cell may be a Minotti's or any other form of Daniell's element, and it should be set apart and not used for any other purpose except for testing, so that its electromotive force may remain fairly constant. The method of testing is as follows: Join up the battery to be tested in circuit with a high resistance and a galvanometer, and note the deflection D, just as in the previous test. If the resistance of the galvanometer is of itself so great that the resistance of the cell is insignificant as compared with it, then it will be unnecessary to introduce any other resistance into the circuit. Next substitute the standard cell for the battery to be tested, keeping the rest of the circuit unchanged, and note the deflection D′ given by it. In this case also the resistance external to the cell should be exceedingly large as compared with that of the cell itself. Then

$$\text{EMF of battery} = \text{EMF of standard cell} \times \left(\frac{\mathrm{D}}{\mathrm{D}'}\right).$$

In this way the electromotive force of any given battery can be expressed in terms of that of the standard cell, and it is not necessary for practical purposes to know what the electromotive force

of the standard cell is in absolute units, so long as it can be relied on to remain constant. If it is a Daniell's cell in good condition its electromotive force will be almost exactly one volt.

In the above formula it is assumed that the resistances both of the battery to be tested and of the standard cell are insignificant as compared with the other resistances in the circuit which remain unchanged during the experiment. If, however, the resistances of the battery and standard cell were relatively considerable, the formula would require to be modified as follows: Let R_0 be the sum of the external resistances in the circuit, which is the same both for D and D′. Let r be the resistance of the battery to be tested and let r' be the resistance of the standard cell. Then

$$\text{EMF of battery} = \text{EMF of standard cell} \times \left\{\frac{D\,(R_0 + r)}{D'(R_0 + r')}\right\}.$$

If the quadrant electrometer is used the test becomes exceedingly simple. Let the deflection D be observed when the poles of the given battery are applied to the terminals of the electrometer, and let the deflection D′ be observed when the standard cell is applied, no resistance being used in either case. Then

$$\text{EMF of battery} = \text{EMF of standard cell} \times \left(\frac{D}{D'}\right).$$

If the electromotive force of any one cell is observed to fall below the average, or much below that of a Daniell's cell in good condition, the cell either requires to be refreshed with crystals of sulphate of copper, or has become foul by the deposit of copper upon the zinc plate. When the last happens, the cell should be taken down and cleaned. When only one cell in an otherwise good pile becomes foul, and it is desired to avoid taking down the pile, the foul cell should be short-circuited, and so put out of action.

THOMSON AND JENKIN'S AUTOMATIC CURB-SENDER.

By J. A. Ewing.

(Communicated by Prof. Fleeming Jenkin, F.R.S.)

§ 18. The object of the automatic curb-sender is to diminish the retardation of signals in long cables caused by inductive embarrassment. This is effected by making each signal be produced, not simply by one current, as in ordinary sending, but by two currents, the second of which is opposite in name to and of somewhat shorter duration than the first. The number of currents is not necessarily limited to two, but for the present purpose it will suffice to consider the case of a signal produced by two currents only.

§ 19. In the Proceedings of the Royal Society for 1855, Sir William Thomson showed how the effect at the distant end of a cable, caused by the application of a battery at one end, could be calculated and represented graphically in what is called the "curve of arrival." After contact is first made at the sending end between the cable and one pole of the battery (the other pole being to earth), a certain interval of time elapses before any effect is felt at the distant end. This interval of time is denoted by the letter a.* After the interval of time a has passed, a current begins to

* Sir William Thomson shows that the value of a in seconds is $\frac{kcl^2}{\pi^2}\log_\epsilon\left(\frac{4}{3}\right)$ where k and c are the values in electrostatic units of the resistance and capacity per unit of length, and l is the length. A consideration of the dimensions of the two systems of units shows that the formula remains unchanged when k and c are expressed in electromagnetic units. Since 1 ohm $= 10^9$ electromagnetic units of resistance, and 1 microfarad $= 10^{-15}$ electromagnetic units of capacity, we have

$$a = \frac{\mathrm{RC}l^2}{10^6\pi^2}\log_\epsilon\left(\frac{4}{3}\right)$$

where R is the resistance per knot in ohms, C the capacity per knot in microfarads, and l the length in knots. Thus a in seconds $= \cdot 000000029\ \mathrm{RC}l^2$. If R′ and C′ be the total resistance and capacity respectively, $a = \cdot 000000029\ \mathrm{R'R'}$. For the Direct United States Cable of 1875 (2420 knots), whose total resistance is 6980 ohms, and capacity 991 microfarads, a would be ·202 seconds; for the artificial cable in the Physical Laboratory of Glasgow University, $a = \cdot 144$ seconds; for the French Atlantic Cable, $a = \cdot 245$ seconds; for the Suez-Aden Cable, $a = \cdot 233$ seconds. Sir William Thomson's theoretical results were experimentally verified by Professor Fleeming Jenkin (*Phil. Trans.* 1862). For fuller information on the speed of signalling, see Jenkin's *Electricity and Magnetism*, pp. 327—333, and *Phil Mag.*, June 1865.

issue from the cable at the receiving end, and increases in strength very rapidly. After a further interval of $4a$ or after a period of $5a$ from the first application of the battery, it attains about half its maximum strength, and there is very little sensible increase in strength after a time equal to $10a$ has elapsed. The curve of arrival is drawn by taking distances along O X (fig. 10) to represent intervals of time, and distances along O Y to represent strengths of

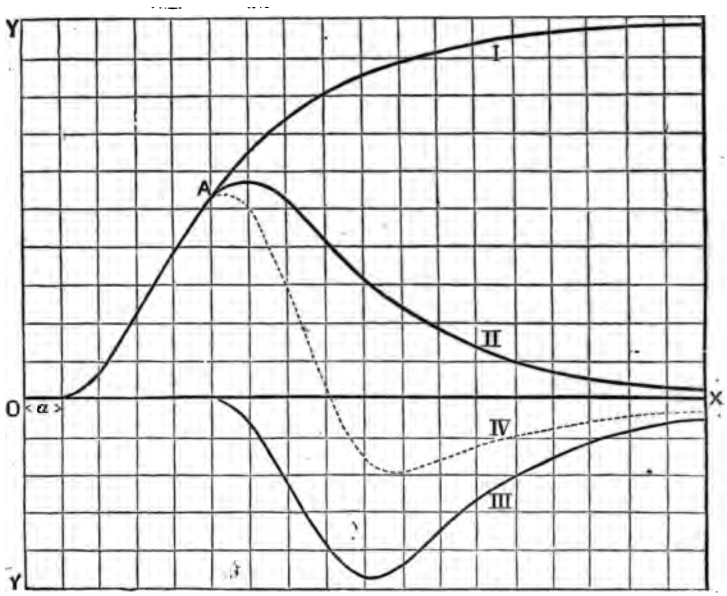


Fig. 10.

current. Curve No. I. shows the gradual increase in strength of the received current at one end of a cable when the battery is applied to and kept in contact with the other end. For a distance corresponding to the interval of time a the curve does not sensibly deviate from the straight line O X; in other words, no effect is observable at the receiving end during this time.

If now, instead of being continuously applied to the battery at the sending end, the cable had been applied to it during a short interval of time, and then disconnected from the battery and connected to earth, the curve of arrival would be of the form shown

by curve No. II.* Curve No. II. shows the effect of applying the battery during a length of time equal to $4a$, and then putting the cable to earth. It will be seen that a current gradually diminishing in strength continues to flow out of the cable at the distant end for a considerable time after the battery has been disconnected. This continued discharge is what gives rise to the difficulty experienced in reading the signals sent through long cables.

§ 20. The principle of "curb" sending is to check this discharge by sending into the cable a second current which will neutralise the bad effects of the first. Thus, let the cable instead of being put to earth after having been in contact with one pole of the battery during the time $4a$, be put in contact with the opposite pole of the same battery for an interval of time equal to $3a$, and then be put to earth. The second contact would, if it had taken place alone, have produced a current at the distant end represented by Curve No. III. The joint effect of the two opposite currents—the first for an interval of time $4a$, and the second immediately following it, and lasting for an interval of time $3a$—will be to produce a received current represented by curve No. IV., whose ordinates are the algebraic sums of the ordinates of II. and III. Curve No. IV. thus represents the curve of arrival given by a signal current of duration $4a$, followed by an opposite or curb current of duration $3a$.

§ 21. The curve of arrival for any current or combination of currents is actually traced on paper by the siphon recorder. Since the deflection of the siphon is sensibly proportional to the strength of the current at any instant, its deflection will correspond to the distances measured in the direction of O Y, and since the paper moves at a uniform speed, and in a direction at right angles to the the direction in which the siphon is deflected, the distances it runs will measure intervals of time, and will correspond to distances measured along O X. Hence the curve traced by the point of the

* The falling curve is of exactly the same form as the curve of arrival, and the actual curve, showing the arrival of an impulse, is obtained by superposing the one upon the other. Thus the distances between curves I. and II. are equal to the ordinates of curve I., if the former are taken as far from the point A as the latter are taken from the point at which curve I. leaves the line O X.

siphon will represent the curve of arrival, and all theoretical considerations respecting the curve of arrival will apply equally to the practical form of the curve drawn on the paper slip.

The curve of arrival, traced according to the considerations in § 19 and § 20, is the curve of arrival in the case where the line is worked directly, without condensers at either end. The effect of introducing condensers is to convert what was formerly a continuous current into an impulse. The curve produced at the receiving end by the application and continued contact of a battery at the sending end, would no longer be of the form of curve I. fig. 10, but would soon reach a maximum, after which it would fall back towards the zero line. It will be readily seen that this would of itself cause

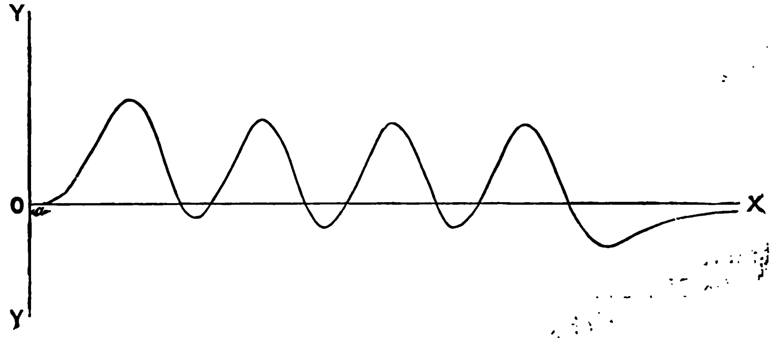


Fig. 11.

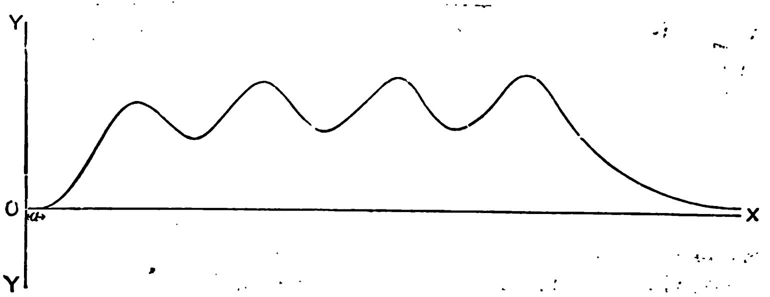


Fig. 12.

something of a curbing effect on signals, since the curve due to a short application of the battery would now come faster back to the zero line than formerly. This effect of condensers is partly the

reason why signals are so much sharper and more legible when condensers are used than when a line is worked direct. Of course, the application of a reversed current to curb the signals is advantageous when condensers are used as well as when they are not.

§ 22. The advantage of curb sending, in giving a sharp outline to the signals, and in bringing the siphon of the recorder or the spot of light from the mirror back (wholly or partly) to zero between successive signals, will be seen by a comparison of figs. 11 and 12.

Fig. 11 shows the theoretical form of the letter H (four deflections above the zero line) when each deflection is produced by a signal current of a duration equal to $4a$, followed by a curb (*i.e.*,

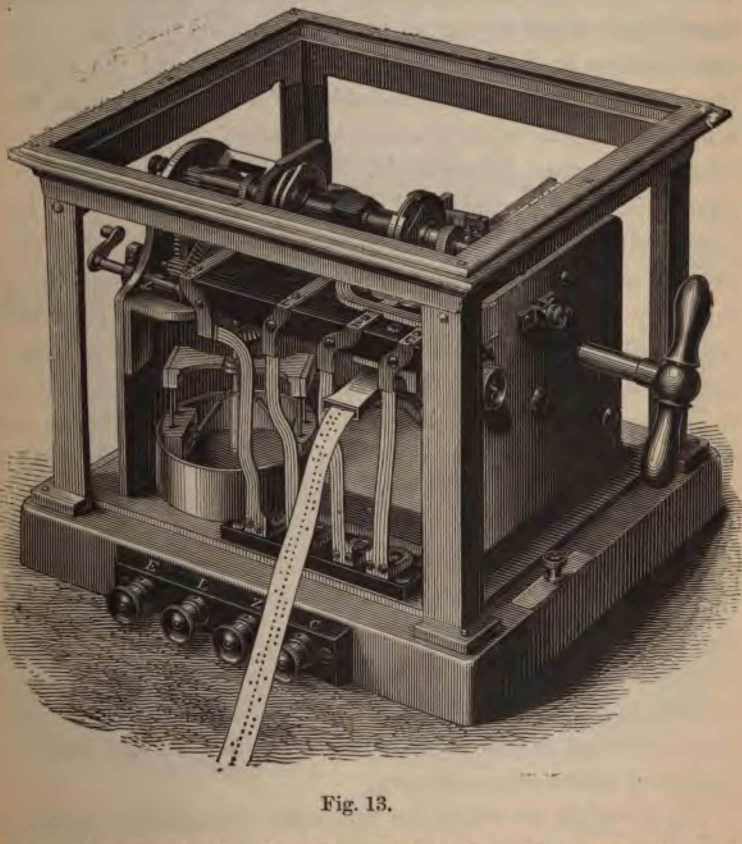


Fig. 13.

a reversed) current of a duration equal to $3a$. This proportion of curb is unnecessarily large for so low a speed, as is shown by the

fact that the curve is brought back past the zero line between the signals.

Fig. 12 shows the form of the same letter, sent at the same speed and under the same conditions, except that the signals are uncurbed. In this case each of the applications of the battery lasts for a time equal to $4a$, and the cable is put to earth at the sending end between the signals for a time equal to $3a$, instead of having a reversed current sent into it during that time. Thus in both cases the total interval of time used in making each signal is $7a$.

§ 23. Such a speed as this (one signal per $7a$) would be very slow in the case of a long cable. The actual rate of transatlantic signalling is about one signal per $1{\cdot}5a$. At this speed the uncurbed curve shown in fig. 12 becomes indistinct, and the successive impulses are barely distinguishable. The curbed signals, on the other hand, give a curb somewhat resembling that in fig. 12, where the return towards the centre or zero line between the signals is only partial, and not complete, as in fig. 11, but is immensely greater than what is observed when uncurbed signals are sent at the same speed. This greater degree of legibility produced by curbing enables the speed to be increased. Experiments with the artificial cable in the physical laboratory at Glasgow appear to show that by using curbed signals, and transmitting them automatically, as high a speed as one signal per a can easily be attained on a long line. In other words, the gain in speed due to the use of the automatic curb sender appears to be at least fifty per cent.

§ 24. In order that curb sending should be successful, it is indispensable that the contacts which give rise to signal and curb currents should be made and broken at perfectly definite instants; in other words, perfectly correct spacing is required. It has hitherto been found to be impossible to obtain this by the use of hand keys, but it is possible to have perfectly correct spacing by the aid of automatic machinery.

The principle on which the automatic curb-sender works is as follows:—

The message to be transmitted is punched on a slip of paper in

right and left holes corresponding to the dash and dot of the telegraphic alphabet. A line of central holes is also punched to facilitate the drawing along of the paper. The punched slip is put into the sender, and carried along at a uniform rate by clockwork. When either a right or a left hole passes under one of two prickers, the corresponding pricker descends into the hole, and by doing so lifts the end of a spring into the rim of a wheel, which revolves once during the time occupied in the passage of one space in the punched paper. The spring so caught remains in the rim of the wheel during a complete revolution, and while it remains there makes an electrical connection between the battery and another set of springs. The latter set are acted on by a double cam, which revolves in the same time with the above-mentioned wheel, and by the contacts made during its revolution sends first the current

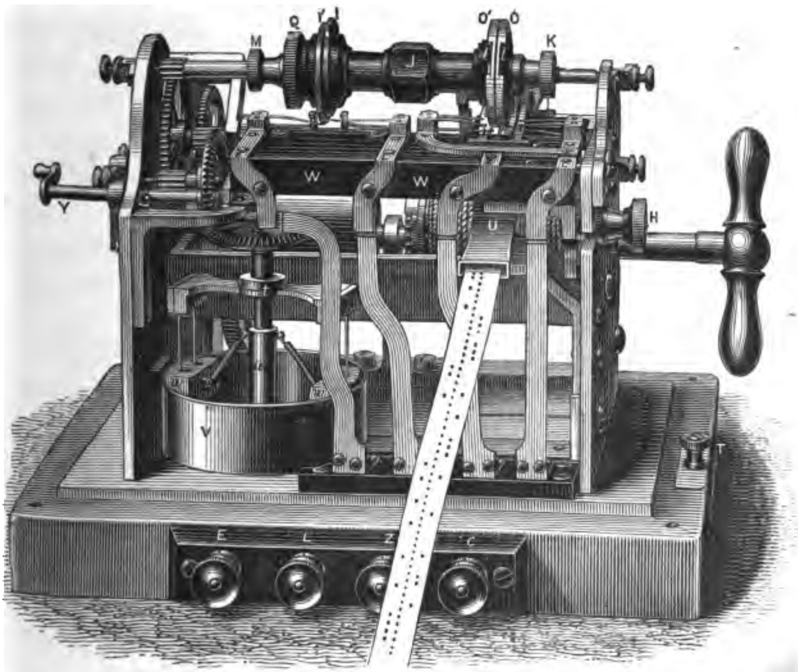


Fig. 14.

from one pole of the battery, and then that fro[illegible] [illegible]ther po[illegible]
during a somewhat shorter time, into the c[illegible] [illegible]

acted on by the pricker on the left hand is raised, the first current is that from the copper pole, and the second current that from the zinc pole; if it is the pricker on the right that has entered a hole in the paper, the sequence of currents is opposite to that just given. Thus an operation of reversal of currents takes place during the passage of every space in the paper, but whether the signal current is to be "copper" and the curb current "zinc," or *vice versa*, is determined by whether the pricker has fallen into a hole on the left side of the paper or on the right.

Figs. 13, 14, and 15, which are engraved from photographs, give three general views of the instrument. In figs. 14 and 15, the glass case and the standards which carry it have been removed, in order to allow the works to be better seen.

§ 25. The Driving Power.—The motion is kept up by the descent of a weight, which has occasionally to be wound up by hand. In order that the driving power may continue during the time taken to wind up the weight, the power is communicated to the machinery not directly from the drum on which the cord of the weight is wound, but indirectly through a spring which is kept in a partially wound-up state during the descent of the weight, and which gives out the energy so stored up in it during the time that the weight is being wound up. This secures an approximately uniform driving power even during the winding up of the weight.

The arrangement will be understood by reference to fig. 16.

The axle A A′ which carries the spring box S and the drum D on which the cord attached to the weight is wound, is divided at p into two parts capable of revolving independently of one another. A ratchet wheel R′ is so arranged that when the drum D is turned by means of the handle so as to wind up the weight, the part A′ p moves independently of A p, and during that time the machinery is driven by the spring in S. When the weight descends, however, causing A′ p to revolve in the opposite direction to that of the winding up, the ratchet wheel R′ no longer allows the motion of A′ p to take place alone, but makes A p be carried round with A′ p. This winds up the spring until the force exerted by the tension of the spring exceeds the resistance of the machinery to

motion. When this happens the spring box S also revolves, driving the machinery. Thus before the weight can begin to produce motion of the machinery, it must first wind up the spring

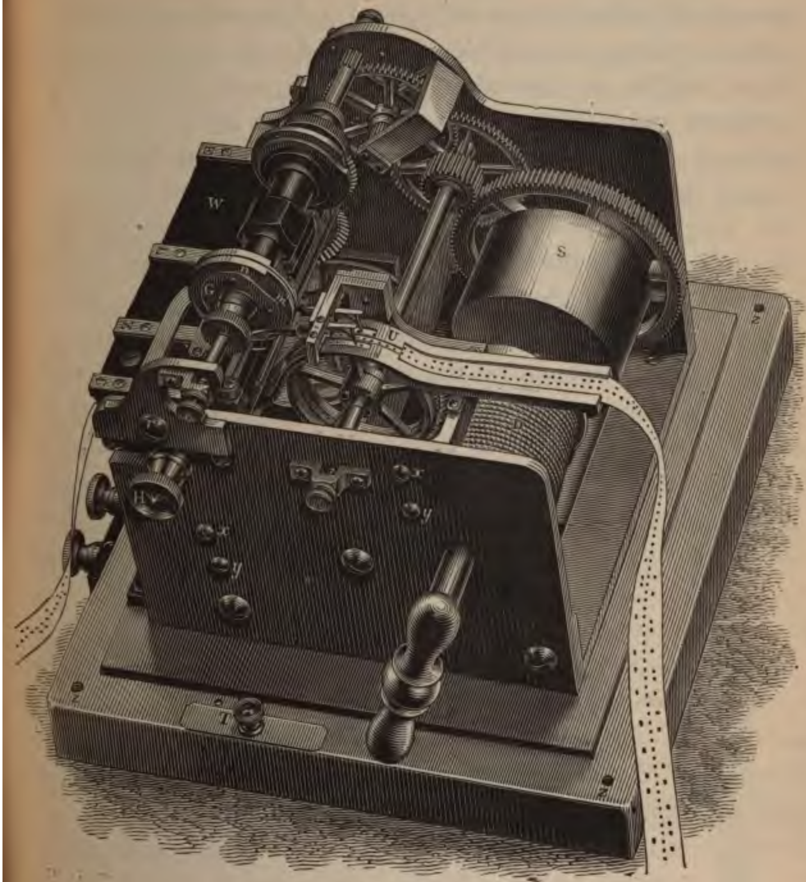


Fig. 15.

in S to a certain extent, and the store of energy thus accumulated suffices to drive the machine at an approximately uniform rate while the weight is being wound up. A second ratchet wheel R prevents the spring from running down without driving the machinery while the weight is being wound up.*

§ 26. THE GOVERNOR.—The speed of the machinery, and con-

sequently the speed of transmission of signals, is regulated by means of a friction governor (figs. 13 and 14.) In the form of governor shown there is a vertical revolving spindle, to which motion is communicated by means of a pair of bevel wheels. To the spindle is fixed a cross bar *t* (fig. 14), from each end of which a weight *w* hangs by flexible springs. As the spindle revolves these weights are carried round with it, and their centrifugal tendency causes them to press against the inside surface of a ring or cylindrical box V. This pressure causes friction, and checks the motion of the machine. The centrifugal tendency of the weights is resisted by a couple of springs, which pull them inwards towards the axis. So long as the centrifugal force of the revolving weights is insufficient to overcome the tension of the springs and to force the weights against the ring, the governor does not oppose the motion of the machinery, which therefore becomes accelerated, until the centrifugal force of the weights become sufficient to bend or extend the springs to such an extent that the weights rub against the ring. The friction so produced prevents any further increase of speed from taking place. If now the tension on the springs be by any means increased, the

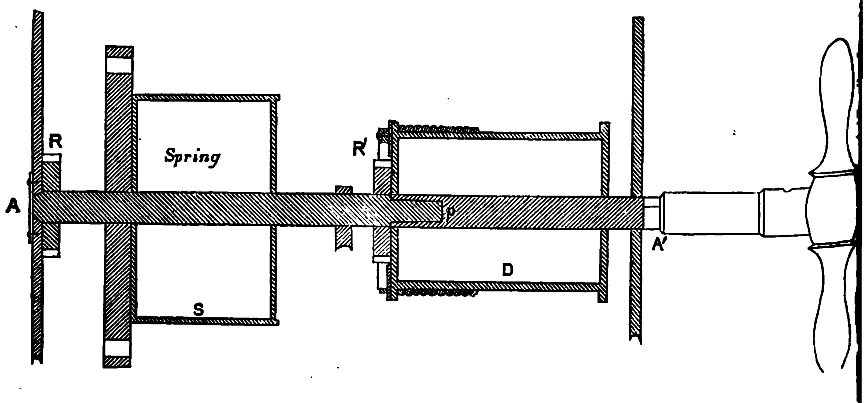


Fig. 16.

amount of centrifugal force required to produce the distension necessary to allow the weights to press against the ring is increased also. Hence the speed will be increased. Similarly, by lessening

the tension on the springs, the amount of centrifugal force required to distend them is diminished, and the speed is consequently reduced. This affords a means of altering the speed, which is effected in the following manner. The ends of the springs which tend to pull in the weights are fixed to a collar u which slides up and down on the spindle. This collar is connected by means of levers to the handle T, the movement of which causes the collar to slide up and down. When the collar u is raised, the tension on the springs is increased, and therefore the speed is increased also. When the collar is lowered, the tension on the springs, and therefore the speed, is lessened.

In another form of friction governor, the motion of the machinery is communicated to the vertical spindle by means of the rolling contact of two discs at right angles to one another, and the whole framework of the governor is supported by means of a contrivance called a geometrical slide,* which enables it to move freely vertically up and down, but in no other direction. The revolving spindle carries the weights, as in the former case. When the framework is moved up, the horizontal disc attached to the revolving spindle of the governor approaches the centre of the vertical disc attached to the machinery. Consequently the latter rotates faster relatively to the former, and the machinery will revolve very fast before the spindle revolves fast enough to cause the weights to press outwards against the ring. In this form of governor the tension of the springs which hold in the weights is kept unchanged, so that the rate of revolution of the spindle of the governor remains constant; but the rate of revolution of the machinery relatively to that of the spindle is changed by altering the height of the whole framework of the governor. This is

* The geometrical slide has five bearing points, each of which is free to move upon the surface on which it rests. The sixth point, which keeps the framework of the governor in equilibrium, is the point in which the vertical disc is touched by the horizontal disc. This mode of support gives rise to a couple tending to cause the whole framework of the governor to revolve about a horizontal axis, and this couple is balanced by the moment of the pressure between the two discs about the same axis. Thus the pressure between the discs is kept at a constant amount for all different positions into which the framework of the governor may be brought for different adjustments.

effected by means of a suitable handle on the right hand side of the instrument. In this form of governor a further provision is made for increasing the range of possible speed, by altering the tension on the spiral springs which hold in the weights. This is done by means of screws inside the governor, and attached to these spiral springs.

§ 27. The Starting and Stopping is effected by means of the screw Y, fig. 14, on the left-hand side of the instrument, which, when turned in the direction of the hands of a watch, advances so that its end presses against the back of the vertical bevel wheel, which communicates motion to the governor. This jams the wheel, and prevents the machinery from moving. When the screw is turned through about half a turn in the other direction, the wheel is freed, and the machinery is free to run.

§ 28. The revolution of the machinery effects two objects. It carries on the paper by means of a toothed roller or spur wheel, working into a central row of holes in the punched paper, and it causes a spindle to rotate on which cams are fixed, which make certain electrical contacts. The toothed roller which carries on the paper ribbon has sixty teeth, and it revolves once for every sixty revolutions of the axle which carries the cams. Hence the cams make one complete revolution for every space in the paper. The paper has the message punched on it in the form of side holes corresponding to the dot and dash of the alphabet, with a continuous row of central holes or indentations which answer the purpose of holes. The central holes or indentations are required to carry the paper on; the electrifical effects are produced by the

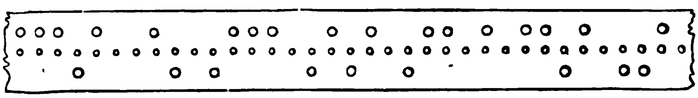


Fig. 17.

side holes only. Fig. 17 shows the appearance of a piece of the punched paper ribbon. A space between two letters is formed by one central hole, and a space between two words by two central holes. (The spaces may also be made in another way, see § 31.)

§ 29. THE PAPER WHEEL, or toothed roller which carries along the paper, has two grooves on its circumference, one on each side of the central row of spurs, and over these the side holes in the paper pass. Fig. 18 shows a front elevation of the paper wheel. Above the grooves stand two *prickers*, one on each side, which descend through the side holes as they pass under them, but which cannot descend except when side holes are passing. One end of a lever is attached to each pricker, the other end of which lifts up a small steel spring when the pricker descends.

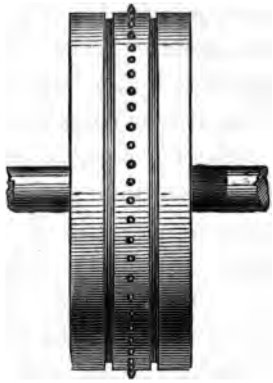


Fig. 18.

In fig. 19, B is the toothed roller which carries along the paper. P is one of the prickers, which is set so as to stand over

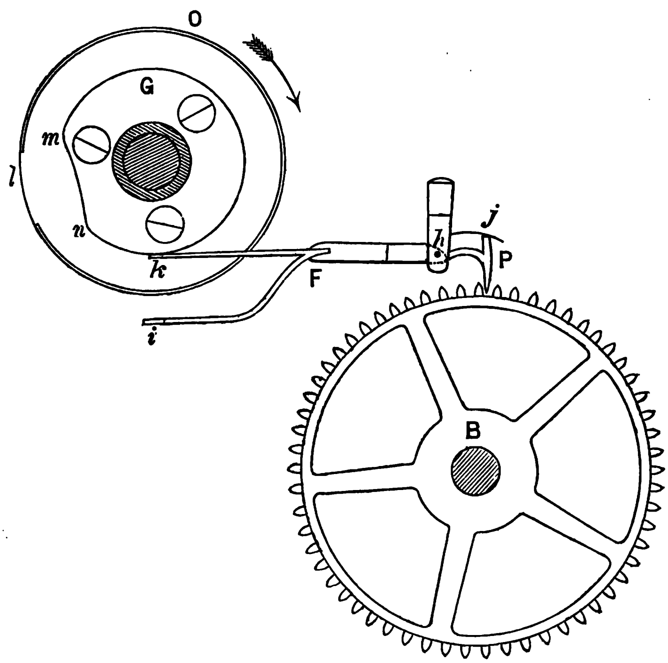


Fig. 19.

one of the two rows of side end holes. Rigidly attached to P, and pivoted at *h*, is the forked lever F, upon the lower end of which, *i*, the end of the above-mentioned spring rests (this spring is not shown in fig. 19; it is marked *a* in fig. 20, below); the other end, *k*, presses up against the rim of the cam G. The spring *j* tends to force P down; but the rim of G prevents *k* from rising, and therefore P from descending. The cam wheel O, of which G is a part, makes one complete revolution in the time taken by the paper to move on through one space. When a side hole in the paper passes under P, P would descend into it if *k* were able to rise. As the cam wheel revolves, a recess, *m n*, in the rim of G passes over *k*. At this point, *k* is free to rise. Simultaneously an opening *l* in the rim of O passes over the end of the spring which rests on *i*. Then, if a hole in the paper is passing under P, P descends, *i* rises, and lifts the end of the spring through the opening *l* into the projecting rim of the wheel O. When *n* passes over *k*, *k* is depressed, and P rises out of the hole in the paper. The spring *a* (fig. 20) remains caught in the rim of O until a revolution is completed, when the opening *l* comes round again, and allows the spring to fall out. By this time the series of operations which makes up the signal is completed. The cam wheel has two sides precisely alike, O and O′, the one corresponding to the pricker which stands over the dot holes, the other to the pricker which stands over the dash holes. The electrical effects produced by the raising of the springs by means of the descent of the prickers will be described further on.

In order that either pricker should descend and raise its corresponding spring into the rim of the cam wheel O, three conditions must be fulfilled. 1st. The opening *l* in the rim of O must be over the end of the spring which rests on *i*. 2nd. The recess *m n* in the cam G must be over the end of the lever *k*. 3rd. A hole in the paper must be passing under the pricker in question. By the construction of the machine, the first two of these conditions necessarily happen together; for the cams O and G are rigidly fixed relatively to one another, and the opening in the one and the recess in the other pass the lowest point at the same time in the revolution. The third condition

must be made to happen at the same time with the other two by adjusting the cam wheel O relatively to the paper wheel B, which is done by turning the former round upon its axle, and clamping it in any desired position. When the instrument is properly adjusted, the recess *m n* will pass over *k* just when the centre of one of the side holes in the paper is passing under P. Then the descent of the pricker, which begins when *m* passes, and its ascent, which is completed when *n* passes, will both take place during the passage of a hole under P, and the pricker will not touch the edges of the hole either in falling or in rising. This prevents the holes in the paper from becoming torn or dragged at the edges, and admits of the same slip of paper being used many times over.

§ 30. The axle which carries the cam wheel O is shown in front elevation in fig. 20. This axle, as we have seen, makes one complete revolution for every space in the paper. O and O′ are the two sides of the cam wheel. This cam wheel, O O′, is called the *determining cam.* The two sides of it are insulated from the axle and from one another by vulcanite. *a* and *a′* are the two springs which are lifted by the lower ends of the forked levers *i i′* into the rim of O O′ through the openings *l l′* when the corresponding prickers descend into the holes in the paper. The springs *a* and *a′* carry small pointers projecting above and below, and

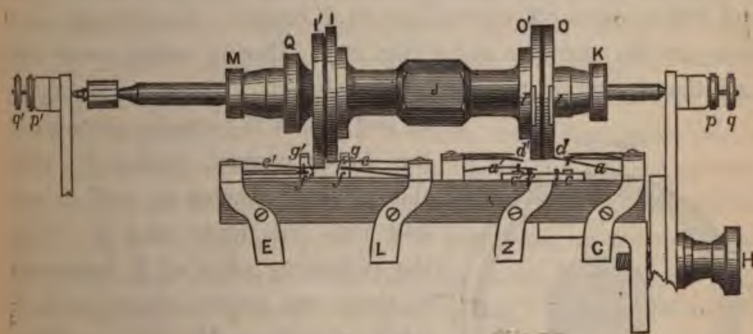


Fig. 20.

tipped with platinum. When a dash, or right-hand side hole, passes, the spring *a* is raised into the rim of O, and remains there during a complete revolution. In the figure it is represented as

being held up in this position. When held up in the rim of O a is kept in contact with the *upper contact spring d*, while the spring a' remains in contact with the *lower contact spring c'*. Similarly, if a dot hole had passed, a would have remained down. and in contact with c; while a' would have been caught up, and held up in contact with d'. So long as no hole in the paper passes, both the springs a and a' remain down in contact with the lower contact springs c and c'.

The zinc pole of the battery is permanently connected to the lower contacts c and c'; the copper pole to the upper contacts d and d'; when, therefore, a dash hole passes, copper is connected to a, and zinc to a'; and when a dot hole passes, copper is connected to a', and zinc to a.

On the same axle with the determining cam wheel, and revolving along with it, are two other cams—I′ and I—called respectively the *signal* and *curb cams*. They are insulated from one another by a disc of vulcanite, and are insulated from the axle and from the determining cam by the vulcanite tube J. They are fixed relatively to one another by a small vulcanite screw. Fig. 21 gives a side-view of the signal and curb cams. The signal cam I′ has a projecting edge extending for rather more than half the circumference; the curb cam I has a projecting edge which begins where the projection on I′ leaves off, and which leaves off where that on I′ begins. Underneath these cams are the springs e and e' (fig. 20). During rather more than half the revolution the projecting edge of I′ depresses the spring e', and causes it to make contact with the lower contact spring f', while the spring e remains up and in contact with the upper stud g. When the projecting edge of I′ has passed e' rises, and makes contact with g', while simultaneously the projecting edge of I depresses e, and holds it in contact with f during the remainder of the revolution. Thus during rather more than half the revolution, e' is

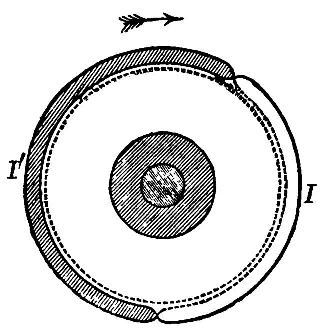


Fig. 21.

in contact with f', and e with g, and during rather less than half the revolution e is in contact with f and e' with g'. These actions depend simply upon the revolution of the axle, and are not affected by the punched paper or by the positions of the springs a and a', but the electrical effects which these actions produce do depend upon the position of a and a', and are therefore determined by the holes in the paper.

§ 31. Electrical Action.—The sping a is permanently in connection with the lower contact springs f and f'. The spring a is permanently in connection with the upper contact studs g and g'. The spring e' is permanently connected to earth, and the spring e to the line. The electrical changes which take place during a revolution of the axle will be understood by reference to the diagram, fig. 22, in which the letters refer to the same parts as in fig 20. In the diagram the spring a is represented as having been raised into the rim of the determining cam by the descent of the right-hand pricker into a hole in the paper. At the instant that a rises, the projecting rim of the signal cam I′ comes round and depresses e'. Thus the copper pole of the battery is connected by way of d, a, f', and e' to earth, while the zinc pole is connected by way of c' a' g, and e to line; and a *negative* current enters the cable to produce the "dash" signal. This state of things lasts for rather more than half the revolution, until the projecting rim of the signal cam I′ has passed and that of the curb cam I has come round. Then e' rises and e is depressed, no change, however, taking place in the position of a and a'. The zinc pole is now connected by way of c', a', g', and e' to earth, while the copper pole is connected by way of d, a, f, and e to line. This sends a *positive* current into the cable to produce the curb to the "dash" signal, which lasts as long as the spring e is depressed by the curb cam. The joint effect of the two currents is, as we have already seen (§ 20), to produce a clearly defined "dash" signal. When the revolution is completed, a falls out of the determining cam through the opening l, and, unless another signal immediately follows that which we have been considering, the copper pole of the battery is disconnected both from line and from earth.

If, instead of a dash hole, a dot hole in the paper had passed,

the pricker on the left side would have descended, and the spring a' would have been raised into the rim of O′, while a remined

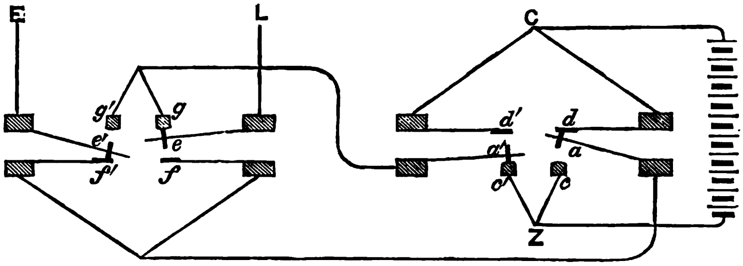


Fig. 22.

down. The reversal of the springs e and e' operated on by the signal and curb cams would have gone on as before. Thus during the passage of I′ the zinc pole of the battery would have been connected by way of c, a, f', and e' to earth, while copper would have been connected by way of d', a', g' and e to line. Similarly, during the passage of I, copper would have been connected by way of d' a', g', and e' to earth, while zinc would have been connected by way of c, a, f, and e to line, There would thus have been a *positive* signal current followed by a *negative* curb current of shorter duration, the joint effect of which would have been to produce a "dot" signal.

The relative lengths of the signal and curb currents depend upon the relative lengths of the projecting rims of the cams I′ and I.* Whether the longer current shall be positive and the shorter negative, or *vice versa*, is determined by the position of the springs a or a' in the rim of the wheel O O′ which is therefore called the *determining* cam wheel. If a succession of holes passes under either of the prickers, the corresponding spring is held up continuously in the rim of the determining cam wheel until all

* With regard to the proportion which the length of the signal cam ought to bear to that of the curb cam, it should be understood that the greater the speed of signalling is relatively to the retardation, or value of a (§ 19), for any cable, the more nearly equal should the signal and curb cams be. If the speed of signalling on different cables be varied so as to be the same relatively to a for each cable, the same pair of cams will be suitable for all.

have passed, because, while the space l is passing, the descent of the pricker into the hole causes the end i of the forked lever to rise, and so prevents the spring from falling out of the rim. If a dot and a dash hole pass simultaneously under the prickers, both the springs a and a' will be raised, and no electrical effects will be produced, for the zinc pole of the battery will be disconnected. Hence two side holes opposite to each other may be used to produce a space, instead of no side hole.

§ 32. THE CONTACT PLATE.—The several contact springs, with their contact pieces and connections, are all fixed on a slab of vulcanite called the *contact plate* W (figs 14 and 15). The contact plate is secured in position by means of the screw H, which causes three projecting points in the framework of the instrument to press against a hole, a V groove in line with the hole, and a plane, in a piece of metal connected to the contact plate (see fig. 20). This mode of support is called a geometrical clamp; it has this advantage, that the plate cannot take up any other than one certain position, and a single screw is sufficient to keep it fixed.

The springs c, c', d, d', f and f' are provided with brass *guard bars* or stops, which limit their range of motion, and prevent them from vibrating when the instrument is running fast, or from sticking to and following the moving springs a, a', e, and e'.

The two halves of the determining cam are insulated from one another and from the axle by vulcanite. Each of the forked levers connected to the prickers is completely insulated. The signal and curb cams are also insulated from one another and from the axle —hence no electrical contacts take place except those which have been considered. The mode in which the signal, curb, and determining cams are fixed to the axle will be understood by reference to fig. 20. J is a vulcanite tube to which the determining cam OO′ is rigidly fixed. The brass screw K on the right of the determining cam clamps it and the tube J on to the axle by jamming them against the shoulder M. The vulcanite screw Q on the left of the signal cam clamps it and the curb cam on to this tube, and therefore to the axle also.

§ 33. SINGLE AND DOUBLE CURB.—In the foregoing description of the electrical action, it has been assumed that the spring e' was

depressed by the signal cam at the same instant that the spring *a* or *a′* rose into the rim of the determining cam. This arrangement would give the whole of the signal current first, followed by the whole of the curb current, and the revolution would begin with the beginning of the former, and end with the end of the latter. A signal may, however, be produced by the joint action of more than two currents, and may represent the resultant effect of three or any number of currents following each other in alternate directions, and being of different lengths.

So long, however, as the form of signal and curb cams described above remains unaltered, there are only two other combinations possible, and these are easily obtained by changing the position of the signal and curb cams relatively to the determining cam.

The arrangement which has hitherto been assumed to exist is called *single curb.* In it the spring *e′* is depressed at the instant that *a* or *a′* rises, and the beginning of the passage of the rim of the signal cam coincides with the beginning of the signal.

If the signal and curb cams are turned round on the axle so as to be slightly *in advance of* the determining cam, it is evident that when either *a* or *a′* rises, the spring *e′* will already have been depressed for some time, and the first current sent will be shorter than formerly, by an amount represented by the distance through which the cams I I′ have been turned forward. The second or curb signal will, however, be as long as before, for the spring *a* or *a′* will continue up during the whole time of the passage of the rim of the curb cam. But since the signal and curb cams are in advance of the determining cam, the rim of the curb cam will all have passed before the spring *a* or *a′* has fallen out of the rim of the determining cam. Hence, before the signal is ended, the rim of the signal cam again depresses the spring *e′* and sends a current into the cable of the same name as that first sent. This is, however, of short duration, for as soon as that fraction of the signal cam's rim has passed which represents the distance by which the signal and curb cams are in advance of the determining cam, the revolution will be complete, the spring *a* or *a′* will drop out of the rim of the latter, and no farther contacts will occur in that signal. There will thus have been three currents produced, the first and

third being of the same name, and the second of the opposite name. The second curbs the first, and the third curbs the surplus curbing effect of the second on the first. Of course the sum of the first and third bears the same relation to the second as the length of the signal cam bears to the length of the curb cam. This arrangement is called *double curb*. Fig. 23 shows the theoretical form of the letter E sent with double curb, the first current being of a duration equal to $3a$, the second of an equal duration, and the third of a duration equal to $1a$, the total time being $7a$, as in the examples previously given. The full line IV. is the resultant signal, of which the dotted lines I., II., and III. are the components.

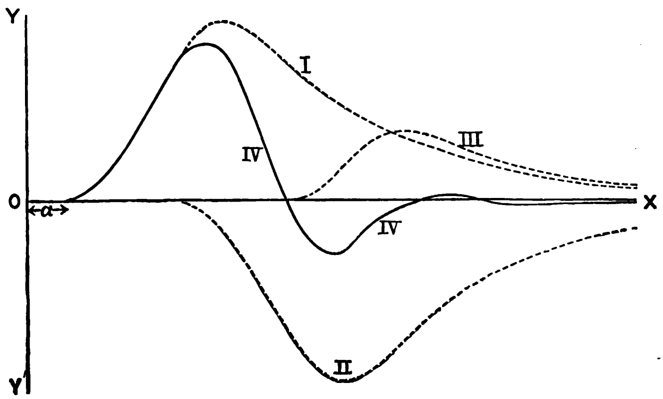


Fig. 23.

§ 34. Preparatory Curb.—If the signal and curb cams are turned back so as to be slightly *behind* the determining cam, then when a or a' rises, e is already depressed, and continues so for a short time, and thus a short curb current is sent first, then the whole of the signal current follows, and lastly the remainder of the curb. This arrangement is called *preparatory curb*. Fig. 24 shows the theoretical form of the letter E given by it. The period of the preliminary curb current is $1a$, that of the signal current $4a$, and that of the second curb $2a$, the total period being $7a$ as before.

§ 35.—We are thus able with a given pair of signal and curb

cams to produce three different arrangements of the currents which go to make up a signal. A perfectly curbed signal—that is to

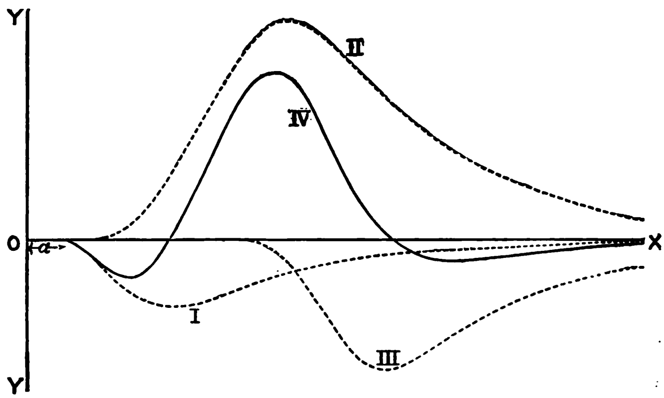


Fig. 24.

say, a signal in which the curve of arrival comes rapidly down to the zero line, and never crosses or leaves it—could only be produced by using an infinite series of curbing currents alternate in direction, and each somewhat shorter than its predecessor. Of course such an arrangement is practically impossible. When the curbing is necessarily imperfect, as it always is in practice, it appears that the arrangement described above as single curb is the best of the three. This is a question which will best be decided by actual trial. It appears likely, however (independently of any consideration of the form of the curve of arrival), that the best means to produce a sharply-defined signal would be to make use of the single curb arrangement, thereby bringing, in the first place, the maximum influence at command to bear upon the indicator (the needle or coil, as the case may be) to deflect it, and then, in the second place, the maximum influence at command to cause it to return to its normal position—always provided that within the limits of a single signal the second influence is insufficient to cause a return much *past* the zero position. This last condition can always be made to hold good by limiting the length of duration of the curbing current relatively to the speed of the signalling.

§ 36. Adjustment of the Contact Plate.—*To put in the Contact Plate.*—Lift off the top of the case and remove the front glass plate. Introduce a piece of the paper ribbon. Start the machinery, and stop it when a piece of the paper in which there are no side holes punched is passing under the prickers. This will cause the ends $i\ i'$ of the forked levers to lie in their most depressed position. Then slip in the plate, taking care that the springs $e\ e'$ are not bent by being pressed against the sides of the signal and curb cams, and that the ends of the springs $a\ a'$ take up their proper positions on the top of the lower ends $i\ i'$ of the forked levers. When one of the three small feet on the side of the framework enters the hole, the other the groove, and the third presses on the plane surface on the piece of metal on the right hand side of the contact plate, the plate will be in its proper position, and is to be secured there by tightening up the screw H.

In removing the contact plate the same precautions have to be observed. The ends $i\ i'$ of the forked levers should be depressed by having a piece of unpunched paper under the prickers.

The platinized contacts on the various springs should be cleaned frequently. They must be kept free from all traces of oil, and should be frequently dusted with a camel's-hair brush. If the contacts made are defective or fail, the points of contact should be cleaned with a very fine file.

Care must be taken that when the springs a, a', e, or e' rise or fall, they do not make contact with both their upper and their lower contact springs simultaneously. If they did so the battery would be short-circuited and sparks would pass which would burn the contacts. If any one of the springs a, a', e, or e' is observed, when rising or falling, to make one contact before it breaks the other, the guard bars which limit the play of the contact springs must be gently bent until this no longer occurs. Two contact plates are provided with each instrument. When the instrument is in constant use the contact plates should be changed every second day, and the one not in use should be carefully cleaned before being replaced.

§ 37. Adjustment of the Determining Cam.—Start the machine with a piece of punched slip in it, and stop it at the in-

stant a side hole is passing under one or other of the two prickers. Then, taking hold firmly of the vulcanite tube J (fig. 20), loosen the brass screw K on the right. Be careful not to apply so much force as to bend or twist the axle. The cams O O′ and vulcanite tube J (as well as the cams I I′) will then be free to move round on the axle. Turn them round until the centre of the openings *l l* in the rim of the determining cam wheel are just over the springs *a a′*. The pricker under which there is a hole in the paper will then be able to descend into the hole, and to raise its corresponding spring into the rim of the determining cam. The centre of the hole in the paper should just be passing under the pricker at the instant that the centre of the opening *l* is passing over the spring *a*. When the cams are turned round into such a position that this happens, secure them and the tube J in that position by screwing up K.

This adjustment is of the utmost importance. When signals fail, the cause is almost always bad adjustment of the determining cam. It is difficult to judge by eye whether the pricker is exactly over the centre of the hole in the paper. When the adjustment has been made, turn the axle slowly round by hand backwards and forwards, and observe whether the prickers tear either the back or front edges of the holes. If the adjustment is perfect, they will fall into and rise out of each hole when the axle is turned either backwards or forwards without touching the paper at either edge of the hole.

§ 38. Adjustment of the Signal and Curb Cams.

1°. *For Single Curb.*—Take hold of the vulcanite tube J firmly with the right hand, and loosen the screw Q with the left. The signal and curb cams I′ and I will then be free to slip round upon the tube J. Put in a piece of punched paper, start the machine and stop it just when one of the prickers is descending into a hole, and is raising its corresponding spring into the rim of the determining cam. Turn the signal and curb cams round until the projecting edge of the signal cam I′ is just depressing the spring *e*. Then secure the cams in this position by tightening up the screw Q. This adjustment is perfect when the depression of the spring *e′* is exactly simultaneous with the rise of the spring *a* or *a′*. The axle

should be allowed to revolve very slowly, by checking its motion with the hand, so that it may be seen whether this adjustment is right; and if not, the cams I and I′ must be shifted round until a state of perfect adjustment is reached.

2°. *For Double Curb.*—Adjust as above for single curb, and then, loosening the screw Q, turn the cams I I′ slightly round on the axle, moving their upper sides away from you as you stand in front of the instrument. In other words, turn the cams round so as to be slightly in advance of their previous position, so that (say) half an inch of the projecting rim of the signal cam I′ has already passed over the spring e' before the spring a or a' rises up into the opening in the rim of the determining cam. Secure the cams in this new position by screwing up Q. The depression of e', which has now occurred before a or a' rises, will produce the second curb of the previous signal (see § 33).

3°. *For Preparatory Curb.*—Adjust as above for single curb, and then, loosening the screw Q, turns the cams I I′ slightly round upon the axle, moving their upper sides towards you as you face the instrument. That is, turn the cams round so as to be slightly behind their position when adjusted for single curb. Then tighten up Q. The spring e' is now not depressed until some time after the spring a or a' has risen, but the spring e is depressed during that time, and thus a short curb current is applied to the cable at the beginning of the signal (see § 34). The distance by which the cams are put back from their position for single curb should not exceed about half an inch measured along the rim.

§ 39. Speed of the Machinery.—The means by which the speed is altered have been described already (§ 26). In the first form of governor there mentioned, which is the one shown in figs. 13 and 14, the speed is altered by sliding backwards or forwards the handle T (fig. 14), which has the effect of raising or lowering the collar u on the spindle of the governor. This increases or diminishes the tension on the springs, which pull in the weights w w. When the tension on the springs is increased, a greater speed is required to give centrifugal force enough to the weights to cause them to diverge and press against the ring or box V. It is not until they press against the box that they

cause any check to the motion of the machine, but as soon as they do press against the box, the friction so produced prevents any further increase of speed. Hence the more tension there is on the springs, the faster will the machine go.

In the second form, the tension on the springs which prevent the weights from diverging is not altered during adjustment, and consequently the maximum rate of revolution of the governor remains constant; but the whole framework of the governor is moved up or down so that the relative rates of revolution of the machinery and of the governor is altered (§ 26). In this form the speed can be still farther altered by varying the tension on the springs which hold back the weights. This can only be done, however, when the machine is standing still. Care must be taken that both weights are held back with equal force, so that when they are caused to diverge by the revolution of the governor, they may both touch the ring at the same time, and press against it equally.

The box of the governor must be kept clear of dust and oil. The weights must not be allowed to scrape against the bottom of the box. They can be raised by loosening the screws which clamp the two pieces of straight spring by which each weight is suspended. These screws must be firmly secured before the machine is started.

No attempt should be made to vary the speed of the machine by altering the driving weights. A weight of about 14 lbs. will generally be found to do well.

All the bearings of the axles must be kept well oiled. If the bearings are too tight, they may be loosened by means of the bearing screws q q' (fig. 20). After loosening the nuts p p' the screws can be tnrned either out or in; and when the desired adjustment is arrived at, all change will be prevented by tightening up p and p'. These and the similar screw bearings on the other axles also afford the means of moving any one of the axles along for a short distance in the direction of its length.

The inner cams of the determining cam wheel (G and G′) *must be kept well oiled*, for otherwise the friction on them of the levers k and k' (fig. 19) will check the running.

§ 40.—Electrical Connections.—The battery power required for the automatic sender is considerably greater than that required in working by hand, not only because the curbing reduces the size of the signals, but also because the speed of sending with the auto is much greater than with the hand key, and consequently the maximum deflection of the coil or needle at the receiving end is a smaller fraction of the whole possible deflection.

When the automatic sender is used, it is of course necessary to provide a hand key, which may be easily thrown into circuit; and for the reasons just given, a separate battery should be used on the hand key of a much smaller number of cells than that used on the auto. No rule can be laid down as to the amounts of battery power. For long cables it will probably be found that twice as many cells are required on the auto as on the hand key.

In order to join up the automatic sender in circuit with the recorder, the connections shown in figs. 8 and 9, have one or other of them to be set up. A switch of the form shown in fig. 25 has next to be provided. By means of the lever handle of this switch, the terminal C can be connected at will either to A or to B. The wire which leads from the sending key to the recorder switch has then to be disattached from the sending key, and attached instead to the terminal C of this second switch. Then from B a wire must lead to the hand key, which is to be provided with a suitable sending battery,

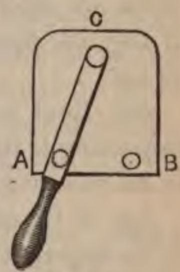


Fig. 25.

and from A to the terminal marked L in the automatic sender. The terminal E of the auto is connected to earth, and the terminals C and Z of the auto are connected to the copper and zinc poles of another (and larger) sending battery. Then by simply turning this switch over to the right the hand key will be thrown into circuit, while by turning it to the left the auto will be joined up instead.

The arrangement of the recorder connections shown in fig. 9 is the most suitable for use with the automatic sender. The automatic signals recorded at the sending station when that arrangement is made use of are very much more easily read than they are

when the system shown in fig. 8 is adopted. In the latter case the curb, or second and reversed part of the signal, comes out nearly or quite as prominently as the first part or signal proper. Thus the letter *e* will appear on the recorder slip at the sending station as two deflections, one above and one below the line, almost exactly equal in amplitude, in form like the letter *a* on the receiving slip, and the letter *a* on the sending slip will appear like *an* on the receiving slip. This makes the sent signals difficult to read; but no such difficulty is felt when the part of the current which records the signals at the sending station is allowed to go to earth there, as it is in the arrangement of connections shown in fig. 9. (See § 14.)

§ 41. Failure of Signals.—If complaints are made that signals sent by the auto occasionally fail to be recorded at the receiving end, that dots and dashes are now and then missed, it will be necessary,—

1st. To look at your recorder slip, and see whether the signals which are reported to have failed at the other end appear there. If they do, then the fault must be at the other end, but if they do not

2nd. Read your perforated slip, and see whether the holes corresponding to the signals which have failed have not been missed in the punching.

3rd. If the failures are still not accounted for, they must be due to the imperfect adjustment of the auto, or to bad spacing in the punched paper.

Stop the machine when a hole is passing under one of the prickers, and just when the opening *l* in the rim of the determining cam is passing over the springs. If now the pricker is fairly over the centre of the hole (see § 37), the adjustment is perfect for that hole, and, if the spacing of the holes in the punched slip is regular, the adjustment must be perfect for *all* holes. In some of the forms of punchers about to be described, the spacing in the paper *must* be perfect, but in another of the forms it is possible that the side holes may sometimes vary slightly in position relatively to the centre holes. Suppose that one of the side holes is slightly behind its proper position. Then at the instant that

the opening l is passing over the springs, the hole in the paper will not be under the pricker as it should be, and consequently the pricker will not be able to descend, and the signal will be missed. It will generally be found possible in this case to avoid having any signals missed by using an *imperfect* adjustment of the auto as follows:—Take a slip of the badly spaced paper, on which some words which you know are punched, and run it through the machine very slowly, checking the revolution by hand. Watch the springs, and read the words by their movements, and notice carefully what signals fail. See whether the failures are caused by the corresponding holes being behind their proper positions, or in advance of their proper positions. If you find that the failures are due to the former cause, then loosen the screw K (fig. 20) and turn the cams slightly backwards, so that when a properly spaced hole is passing, the pricker may descend, not into the centre of it, but near its back edge. Then when one of the holes passes which has been punched slightly behind its proper position, it is almost certain that some part of it,—probably a part near the forward edge,—will be passing under the pricker at the right instant, so that the pricker will descend and the signal be transmitted. Of course if the faulty holes had been *in advance* of their proper position, it would have been necessary to turn the cams slightly *forward*.

4th. When signals are reported to fail, although the prickers enter the corresponding holes, all the contact springs, &c., must be cleaned.

A good plan for seeing whether the prickers are entering the holes is to take a piece of the punched paper and smoke it over by drawing it through the flame of a lamp or candle; then run it through the machine. Every time the recess $m\ n$ in the cam G (fig. 19) comes round, the prickers will descend, and if a solid part of the paper is passing under them, they will leave marks on the smoked paper at the points where they touch it. If these marks are seen close to the edge of the side hole, the prickers have evidently not entered the holes, and the adjustment must be looked to.

§ 42. General Directions.—When the outer case of the in-

strument has to be removed, it should be taken off bodily by taking out the screws at the four feet *z z z z* (fig. 15.) The various brass pieces of the case should not be separated from one another, as a difficulty would probably be experienced in putting them together again.

If it is desired for any reason to remove the paper trough U (figs. 14 and 15), or the prickers, the contact plate must be first removed; then the axle which carries the cams should always be taken off. This is done by taking out the screw *r* (fig. 15), which secures a standard which carries the bearings for the right hand end of the axle. When the screw *r* is taken out, this standard comes off, and the axle is thus easily removed without having its bearings interfered with. The paper trough and pickers can then be removed bodily by taking out the screws *x y x y* on the right hand side-plate of the instrument, which secure the angle-pieces carrying the trough. In this way the trough and prickers can be taken off together without any of the small screws being touched which fix together the supports of the prickers.

Care must be taken that the successive turns of the string wound upon the driving drum D do not overlap each other, for if they do they are liable to come in the way of the projecting spurs on the toothed wheel which carries the paper along.

The string which suspends the driving weight must hang freely, and not chafe against the sides of the hole in which it passes through the table.

PUNCHES.

§ 43. The machines for perforating the paper ribbon used in the automatic sender are of several forms. One puncher is adapted for punching side holes only, and the paper used for it may be prepared beforehand by having a properly spaced row of central holes punched in it. This puncher has three finger keys. When any one of the three keys is depressed, the paper is carried along a step. When the central key is depressed nothing further happens, but when either of the side keys is depressed a side hole is perforated in addition to the paper being moved forward. It is essential that the paper should be at rest when the punch enters

it, and should continue at rest so long as the punch is in it. The machine is therefore arranged so that the movement of the paper happens *early* during the stroke or depression of the key, and the punch is not forced through the paper until the end of the stroke. The manner in which this is effected will be readily understood by an inspection of the machine. The feed motion, by which the paper is carried along, may not, however, be so readily understood without explanation. The paper is carried along by the revolution of a toothed roller, the teeth of which enter the previously prepared central holes. On the same axle with this toothed roller is a ratchet wheel, having the same number of teeth as there are in the roller, so that the paper advances one space when the ratchet wheel is pushed forward one tooth. The ratchet wheel is caused to advance by the following means:—The depression of any of the three keys is made, by means of a spring, to communicate horizontal motion to the pivoted end of a long horizontal paul, the other end of which gears into the upper side of the ratchet wheel. When the key is depressed, this paul is pressed along, and the ratchet wheel advances. The end of the paul which gears into it is thus carried upwards as well as along, and by the time that the ratchet wheel has advanced through the space corresponding to one tooth, the end of the paul has risen so far that the back of the paul presses upwards against a firm stop fixed above it. This prevents any further advance of the ratchet wheel during the remainder of the stroke. The horizontal pressure on the pivoted end of the paul continues, however, and consequently the ratchet wheel (and therefore the toothed roller and the paper) is held firm during the descent of the punch. When the key is allowed to rise, the horizontal pressure on the paul ceases; it is pulled backwards by a suitable spring, and its weight causes the unpivoted end to fall down into gear with the next tooth of the ratchet wheel. The ratchet wheel is prevented from following the horizontal paul backwards by a second paul or click.

In using this instrument, it is not essential to have paper in which a central row of holes has previously been punched. If a slip of plain paper be used, the toothed roller will make a series of central embossed marks or indentations, which will be found to

answer the purpose of central holes sufficiently well, for the paper so prepared will run through the automatic sender, the teeth in the spur wheel of which will enter the successive central indentations in the paper. It will be found necessary, when embossed paper is used, to apply a strong retarding force to the paper as it enters the auto, so as to prevent it from riding on the top of the spurs, instead of being pierced by them. This retarding force is easily applied, in exactly the same way as it is applied in the siphon recorder, by passing the paper under a spring guide, the pressure of which upon the paper will cause friction and hold back the paper. (See *a* fig. 1.)

The depression of the keys of this puncher is most easily effected by the help of light wooden mallets, on the end of which india-rubber is fixed, or, better still, by putting fingerstalls tipped with india-rubber over two fingers of each hand. Care must be taken not to strike a sharp or violent blow, but rather to make the blow as much like a shove as possible. If the blow be too sharp, the paper will not have time to advance through a complete space before the punch enters it, and a misplaced or torn hole will be produced. Violent blows are also very apt to break the springs by which the feed motion is worked. To avoid breakage, multiple springs are used for the dot and dash keys. If the single spring under the centre key should break, it is not necessary to interrupt the work to put on a new spring, for the spaces may be made by depressing both side keys simultaneously.

The bridge which forms a back stop for the keys should be adjusted so as just to touch the backs of the levers, without pressing on them, when they are in their raised position.

§ 44.—The Power-Puncher.—In this form of puncher the heavy work is done by power stored up in the form of a wound-up spring or raised weight, and the operator, by depressing the finger keys, releases a detent, and so allows the stored-up power to take effect. Hence the name, in which the prefix "power" has the same significance as it has in the term "power-loom."

In the power-puncher there are three finger keys—for the dot, dash, and space. When any one of these is depressed, a detent allows one tooth of an escapement wheel to pass. There are

twelve teeth on the wheel, so that the axle which carries it revolves through one-twelfth of a whole revolution for every depression of a key. On the same axle is fixed a cam wheel called the paper cam, with twelve projections on it, which act (as they pass) upon one end of a lever, the other end of which carries forward a ratchet wheel tooth by tooth. On the same axle with the ratchet wheel is a toothed roller which gears into the central row of holes in the paper. The paper is thus moved forward one space whenever any one of the three keys is depressed. The projections on the cam are so designed as to make the paper start and stop slowly at each movement, so as to secure uniform spacing at different speeds. There is a second ratchet wheel fixed on the same axle with the one just mentioned, into which a paul is made to gear just as the punch is entering the paper. In this way the paper is held perfectly steady during the descent of the punches. The feed motion of the power-puncher is identical with that of the single lever puncher, which is described at greater length in § 45.

There are three punches, corresponding to the centre hole and the two side holes. The manner in which they are forced down into the paper is as follows:—On the axle, which carries the escapement wheel and paper cam, there is a second cam wheel fixed, called the punching cam, on whose rim there are twelve wedge-shaped projections. One of these passes over the central punch and depresses it every time that the axle revolves partially owing to the depression of any one of the three keys. If the centre key is depressed, then nothing occurs except the release of the detent, and the consequent partial revolution of the axle, which causes the paper to advance one space, and causes one central hole to be punched. Neither of the side punches is affected, because they are somewhat shorter than the central punch, and so allow the revolving wedges of the punching cam to pass without depressing them. But if one of the side keys is depressed, a packing piece slides in over the top of the corresponding side punch, and, the detent being released as before, the revolving wedge of the punching cam cannot now pass without depressing both the centre and the side punch, which has been (so to speak)

lengthened by the introduction of the packing piece. If both side keys are simultaneously depressed, both packing pieces are slipped in over the punches, and all three holes are punched. The effect of this would be (as we have seen in § 31) to produce a space in the sending.

The spring or weight has to be wound up from time to time. With an ordinary Morse spring, the power-puncher will punch about twenty words without needing to be wound up afresh.

Great care must be taken to keep the fingers clear of the keys which are intended not to be depressed, otherwise the packing pieces may be slipped in accidentally and false signals be produced.

The best way to put in the paper is to double it over the end of a piece of steel spring about six inches long, and push the spring and paper in together—care being taken to draw back the spring before beginning to punch.

§ 45. The Single Lever Puncher.—In this puncher the principle of packing pieces is used, and the feed motion is the same as that of the power-puncher, but the work is entirely done by hand. There is a long horizontal lever, on the end of which nearest to the operator there are three finger keys. When any one of these keys is depressed, the near end of the lever is depressed with it. Fixed to the lever is a projection A (fig. 26), with a cam-shaped surface, and as the lever is depressed this cam slides along the surface of another piece B in contact with it, and gives a reciprocating motion to B. The piece B actuates the feed in the manner described below. When the centre key is depressed the lever descends, works the feed, and a centre hole is punched. When either of the two side keys is depressed, a packing piece slides in over the top of the corresponding side punch; then the lever descends, works the feed, and punches both a side and a centre hole. A spiral spring, fixed to the distant end of the lever, pulls it back when the pressure of the fingers is removed.

The feed mechanism shown in Fig. 26. The paper is carried along by a toothed roller C, whose teeth enter the centre holes. This roller is placed immediately behind the punches. Fixed on the same axle as the roller are two ratchet wheels, D and E, whose

teeth face opposite ways. D is the one by means of which the paper is drawn along; the other one, E, is required in order that

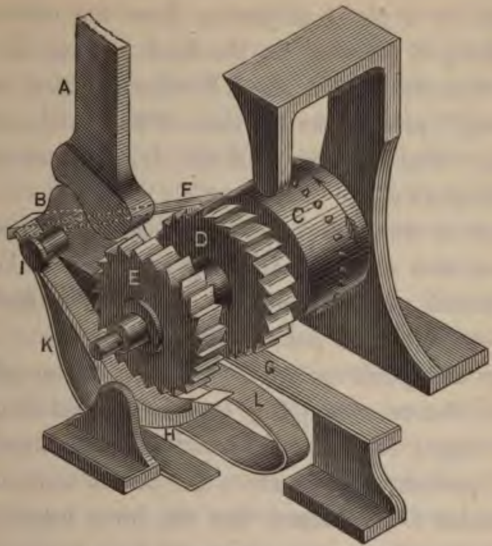


Fig. 26.

the paper may be held so as to be perfectly incapable ot moving during the time that the punches are entering and leaving it. During the early part of the descent of the lever, the piece B descends, and carries with it a spring paul F, fixed on the side of it nearest to the centre of the instrument, and gearing into the upper side of the ratchet wheel D. This paul pulls the ratchet wheel round one tooth, and consequently causes the paper to advance through one space. When the advance is complete, a click G, working on the lower side of the same ratchet wheel, falls into gear, and so prevents the wheel from turning the wrong way during the backstroke. Just before the punch or punches enter the paper, a paul H on the outer side comes into gear with the ratchet wheel E on its lower side. This paul has been gradually rising during the descent of the lever, but it only comes into gear when the paper is just about to be punched. It remains in during the whole time that the punch or punches are in the paper, and insures that the paper does not move during that time. As the

lever is allowed to rise, this paul falls out of gear; the paul F rises so as to catch hold of the next succeeding tooth of D, while the click G prevents the roller from turning backwards. The paul H is actuated by a pin I projecting from the outer side of B. There is a spring K pressing on the back of paul H, in order to disengage it from the ratchet wheel E when the lever rises. There is also a spring L pressing on the back of B, to make it rise when the lever rises. The two springs K and L are fastened by screws under the sole-plate of the instrument.

The surface of the cam A is so shaped as to start the paper gradually, and also so as not to bring any force to bear upon B during the actual punching of the paper. The feed motion is completely over before the punch enters the paper.

The springs and pauls for the feed motion are usually provided in duplicate. Before attempting to take the machine to pieces, the operator ought to make sure that he understands what the conditions of perfect working are. The paul F must begin to pull round the ratchet D the instant that the lever begins to descend. It must cease to pull as soon as the paper has advanced one space, and before the punch begins to enter the paper. At the instant that it ceases to pull, the click G must fall into gear, and at the same instant the paul H must come into action. During the remainder of the descent of the lever, no force must be brought to bear upon B by the cam A, although A continues moving.

ON THE USE OF A SOFT IRON CORE IN SIR WILLIAM THOMSON'S MIRROR GALVANOMETERS.

In working submarine cables it is a standing rule to use the lowest battery power possible that is compatible with the good working of the line. This rule becomes of great importance when a fault makes its appearance. Some three or four years ago I was engaged in working a long cable—that from Singapore to Hongkong, which I believe is, electrically, the longest in the world. To obtain signals sufficiently large, with a moderate battery power, a very sensitive suspension of the mirror was necessary. This was

inconvenient, as the needle oscillated considerably and was a long time in returning to rest.

It occurred to me that if the effect of the current upon the magnet could be increased, we could either use a stiffer suspension, or could diminish the battery power used on the line.

To effect this, I introduced a rod of soft iron into the tube in which the mirror is suspended, and found that the result fully answered my expectations. With the same suspension of the mirror with which we had previously used ten cells (Minotti's) I obtained good readable signals with two. The contrivance, however, is more useful in the case of faulty lines, which by its means can often be worked with a moderate battery power, when, without it, a larger number of cells would be necessary, thus developing the faults more rapidly, and probably breaking the cable down entirely. The soft iron also acts as a damper and checks the oscillations of the needle. I have not seen this idea published in any work on telegraphy, and as it has been adopted over the whole of the Eastern Extension system, where the mirror galvanometers are still used, and has proved to be a success, it may be useful to others engaged in submarine telegraphy.

WALTER JUDD.

Singapore, July 21st, 1876.

THE POROSITY OF DEFECTIVE INSULATORS.

Various experiments made by the Indian Telegraph Department have shown that when an insulator is electrically worthless, although perfect as far as the eye can judge, this defect is caused by the existance of extremely fine pores extending right through the porcelain, and ordinarily containing moisture. It is of the utmost importance that all such defective ones should be eliminated before any batch of insulators is employed in the construction of a line. To do this all the insulators are placed in an inverted position in water and water is poured into the porcelain cups, the water in each case reaching to within a short distance of the rims; the resistance from the water inside to the water outside the porcelain cup is then measured.

Now in the case of first-rate insulators, such as those of Messrs. Shonberg of Berlin, the resistance of even the bad specimens, that is those to be rejected, is often very great. If the rim of a good insulator is slightly damp it will appear to be a bad one, therefore the rims of all the insulators are artificially dried before the testing. In England the rims are dried by running red hot rollers along the top of the insulator trough. If the rollers are not hot enough or not kept long enough over the trough the rims are not dried; if too hot, or allowed to remain too long, the water begins to evaporate, and on removing the roller there is frequently a deposit of damp on the rim of the insulator. All this renders quantitative insulator testing a tedious operation.

Now it occurred to us that it might be possible mechanically to enlarge the pores of bad insulators so as to diminish their resistance, and so greatly to facilitate their detection. We selected two insulators made at Hizen in the south of Japan, one of them known to be good and the other bad. The resistance of each insulator was measured before and after each of the following processes by the loss of charge method, using a Thomson's Quadrant Electrometer and a Condenser, whose capacity was one-third of a farad, the edge of the porcelain cups being in each case very carefully dried.

First. The poles of a powerful Ruhmkorff's coil were attached to the outside and inside of each insulator in succession, and the primary current from a Grove's battery kept on for about six minutes, the hammer of the coil oscillating so as to make and break the primary current with great rapidity during the time. The good insulator was unaffected, but the resistance of the bad one was reduced to less than one-half.

Secondly. Both insulators were soaked for several days in a saturated solution of copper sulphate, then taken out and left to dry so as to allow the copper sulphate, by crystallizing in the pores of the bad insulator, to enlarge them; the soaking and drying were repeated, and they were then soaked in fresh water for some time. The good insulator was still unaffected; the resistance of the bad one was reduced to one-fourth its original value.

Thirdly. Two other insulators, one known to be good and one bad, after being soaked in ordinary water, were kept at a

temperature of about 10° C. for fourteen hours by means of a freezing mixture, then soaked in lukewarm water for three hours, again frozen and soaked in ordinary water. The good insulator was unaffected, the resistance of the bad one was reduced to one-third. The following table shows the resistance of the four insulators under the various circumstances.

RESISTANCES IN MEGOHMS.

	Initial.	After application of Ruhmkorff's coil.	After soaking in Cu SO_4 and crystallizing.	After freezing.
No. 1	Infinite	Infinite	Infinite	
No. 2	290	129	75	
No. 3	Infinite	—	—	Infinite
No. 4	165	—	—	56

Infinite resistance means that the insulator retained a positive or negative charge for several minutes without any perceptible loss.

Since connection with the outside of the insulator is made by means of a copper wire placed in the water of the trough, and with the inside by a copper wire attached to the iron stalk, we have two different metals, copper and iron, separated by the partially conducting porcelain; the insulator, therefore, acts as a simple cell, and we are enabled to measure its resistance in two distinct ways. (1.) By giving an independent charge from a Daniell's cell and taking time readings with the electrometer as the charge is lost, using as the zero the reading when there is no further loss. (2.) Taking time readings on insulation after short circuiting, as the insulator's own electromotive force causes the copper wire attached to the iron stalk to acquire a negative potential relative to the copper wire attached to the outside of the

insulator. In both cases we must calculate the resistance after a certain time has elapsed, when the ratios between successive readings at equal intervals of time have become equal.

We are inclined to think that a sudden cooling of porcelain insulators from some moderately high temperature, which might be determined by experiment in the manufactory, would so entirely destroy the insulation of bad specimens that they might be separated from the good with great facility. Such a process could most readily be carried out by the manufacturers. The authors of this paper have at present no opportunity of practically testing at a manufactory the importance of their suggestion. The experiments, however, described in this communication prove conclusively that the minute fissures in bad insulators may be artificially enlarged. It is easy also to see how the fact that bad insulators act as simple voltaic cells may be employed in separating them from good ones.

J. Perry.
W. E. Ayrton.

The Imperial College of Engineering,
Tokei, Japan.

A NEW METHOD OF TAKING THE LOOP-TEST.

London,
October 18th, 1876.

Dear Sir,

Under the instructions of Mr. W. F. King, Engineer-in-Chief to the Western and Brazilian Telegraph Company, and while localising faults on board that Company's repairing ship "Norseman," I found the following method of loop-test give more accurate results than any of those mentioned in Clark and Sabine's book on Electrical Tables and Formulæ. If you find that it has not already been published, perhaps you will kindly have it inserted in the next number of the Society's Journal.

Instructions.

Connect up the testing-battery for quantity, so as to have a *low* internal resistance, and see that it is *very well insulated*, by having the jars encased or supported in dry solid paraffin or otherwise. Arrange the testing instruments and leads as in fig. 1, connecting one side of the galvanometer to a fixed terminal somewhere between the ends of a series of resistance coils, or to the shifting pointer in a set of slides, and the other side to earth, the ends of the cable being connected to the ends of the resistance coils or slide, as shown in the figure :—

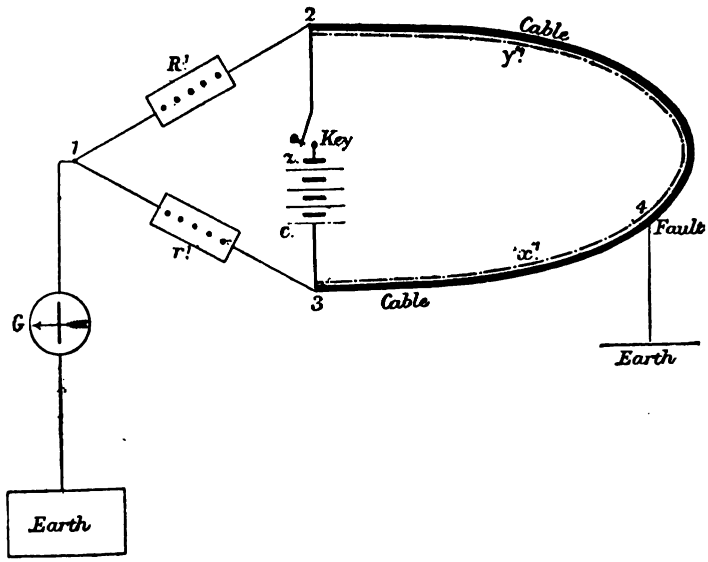


Fig. 1.

Get a balance with resistances r_1 and R_1 as nearly as can be judged in amount to that of x_1 and y_1, the respective resistances at the time from the ends of the cable to the fault, the zinc pole being connected to terminal (2).

Then
$$\frac{r_1}{R_1} = \frac{x_1}{y_1}.$$

Now reverse the ends of the cable and procure another balance,

as represented by fig. 2 (zinc still to 2), with r_2 and R_2 as the resistances in circuit.

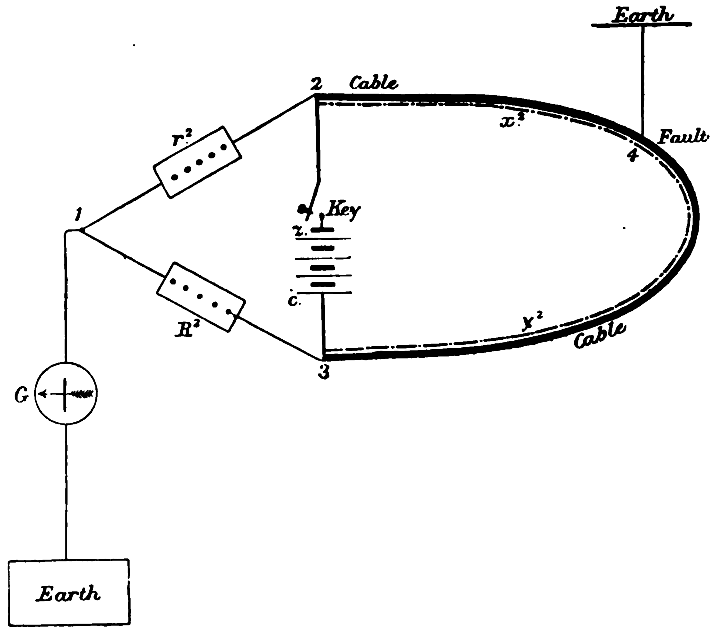


Fig. 2.

Then
$$\frac{r_2}{R_2} = \frac{x_2}{y_2}.$$

And if the fault remains constant,
$$\frac{x_1}{y_1} = \frac{x_2}{y_2} = \frac{X}{Y}.$$

Therefore taking the mean of our two balancings, we get
$$\left(\frac{r_1}{R_1} + \frac{r_2}{R_2}\right) \times \frac{1}{2} = \frac{X}{Y};$$

$$i.e.\ \frac{r_1 R_2 + r_1 R_2}{2 R_1 R_2} = \frac{X}{Y} \therefore X = \frac{Y(r_1 R_2 + r_1 R_2)}{2 R_1 R_2},$$

or the distance in *ohms* of the fault from that end of the cable marked 3 in fig. 1.

The great advantage of this method seems to be that the same amount of current passes through the galvanometer as out at the fault.

By trying quick reversals and getting a few means, very accurate results can be obtained, only the resistance of the fault must not be very high.

Yours faithfully, ANDREW JAMIESON.

J. Sivewright, Esq., Secretary,
Society of Telegraph Engineers.

To find the diameter λ of a wire to fill a bobbin of given dimen sions (outer diameter A, inner diameter a, length b) and produce a given resistance R, allowance being made for the radial thickness ρ of the insulating covering :—

$$\text{Length of 1st coil} = 2\pi\left(\frac{a}{2} + \rho + \frac{\lambda}{2}\right),$$

$$\text{,, of 2nd ,,} = 2\pi\left(\frac{a}{2} + 3\rho + \frac{3\lambda}{2}\right),$$

$$\text{,, of two together} = 2\pi\left(2\frac{a}{2} + 4\rho + \frac{4\lambda}{2}\right),$$

$$\text{,, of } n \text{ coils} = 2\pi\left(n\frac{a}{2} + n^2\left(\rho + \frac{\lambda}{2}\right)\right),$$

$$= 2\pi n\left(\frac{a}{2} + n\left(\frac{2\rho + \lambda}{2}\right)\right),$$

$$\text{Number of coils} = \frac{A - a}{2\,(2\rho + \lambda)} = n.$$

$$\text{Total length of 1 layer} = \frac{\pi\,(A - a)}{2\rho + \lambda}\left[\frac{a}{2} + \frac{A - a}{4}\right],$$

$$= \frac{\pi\,(A - a)}{2\rho + \lambda}\left[\frac{A + a}{4}\right].$$

$$\text{Number of layers} = \frac{b}{2\rho + \lambda},$$

Therefore total length is—

$$L = \frac{\pi\, b\,(A - a)\,(A + a)}{4\,(2\rho + \lambda)^2},$$

Now

$$b\,R = c\frac{L}{\lambda^2}.$$

and

$$L = \frac{R\,\lambda^2}{c}.$$

therefore

$$\frac{R\lambda^2}{c} = \frac{\pi b(A-a)(A+a)}{4(2\rho+\lambda)^2},$$

therefore

$$(\lambda^2 + 2\rho\lambda)^2 = \frac{\pi b c(A-a)(A+a)}{4R},$$

therefore

$$\lambda^2 + 2\rho\lambda = \sqrt{\frac{\pi b c(A-a)(A+a)}{4R}},$$

adding ρ^2 to both sides

$$\lambda^2 + 2\rho\lambda + \rho^2 = \rho^2 + \sqrt{\frac{\pi b c(A^2-a^2)}{4R}},$$

From which

$$\lambda = -\rho + \sqrt{P\rho^2 + \sqrt{\frac{\pi bc(A^2-a^2)}{4R}}},$$

or if $k = c\,\frac{\pi}{4}$,

$$\lambda = -\rho + \sqrt{\rho^2 + \sqrt{\frac{kb(A^2-a^2)}{R}}},$$

when k is the resistance of a wire $\frac{\pi}{4}$ units in length and one u in diameter.

[1 P] R. S. Brough.

ON A NEW FORM OF JOINT FOR COVERED WIRES.

Since the discussion which followed the reading at the Society of Telegraph Engineers in November last of Mr. Mance's paper on the making of joints in gutta-percha covered wires, in which I took part, I have endeavoured to supply what appeared to be much desired, viz., a simple, cheap, and efficient mode of joint making, and, having succeeded far above my expectations, I lose no time in bringing it before the notice of the Society. That the joint is a simple one and can be readily made by inexperienced hands will be seen by the instructions for making the same, which are as follows. Remove the gutta-percha from the ends of the wires to be joined, either by heating or by the knife, the former method is the best, clean the copper wire with emery paper, and make a short "bell-hanger's twist."

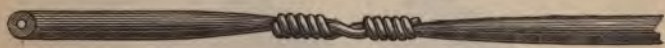


Fig. 1.

If the wires are thoroughly cleaned and the twist tight and well made no solder will be required. Warm the surface only of the gutta-percha and the copper at the joint by means of the spirit lamp, and apply the compound in a warm state until the copper is covered slightly thicker with it than with the gutta-percha, then while the compound is warm place it on the bottom half of the wooden or vulcanite mould, and place the top half over it; then

Fig. 2.—LOWER SECTION, JOINT OPEN.

UPPER SECTION, JOINT OPEN.

applying the clamp, screwing it firmly down until the surfaces of the two halves press firmly against one another as shown in fig. 3.

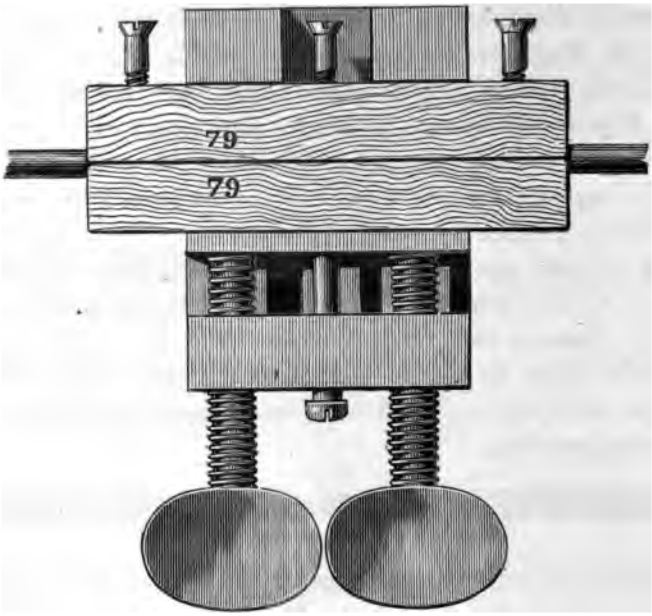


Fig. 3.—SIDE VIEW OF CLAMP.

Next insert the screws in the top half of the wood or vulcanite mould, and after they are firmly screwed home remove the clamp, and the joint is complete (fig. 4).

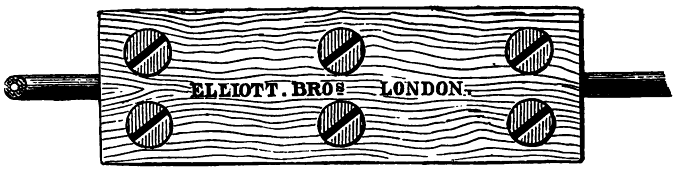


Fig. 4.—JOINT COMPLETE.

As to the durability of such a joint I have no doubt. The electrical qualities, as far as I have had time to test the same, are highly satisfactory. Ten joints were made, in short lengths of ordinary No. 7 gutta-percha covered wire, by a man who never made a gutta-percha joint, and immersed in water and tested on

the following dates by the accumulation test. Battery one hundred cells. Time one minute. Standard two yards of the same sized core = 1.

No.	February 16.	February 23.	March 1.	March 16.
1	0·83	0·66	0·66	1·00
2	0·83	0·66	0·66	0·83
3	0·66	1·00	0·66	0·66
4	0·66	0·66	0·66	0 83
5	0·66	0 66	0·66	0·66
6	0·83	0·66	0·66	0·66
7	0·66	0·66	0·66	0·83
8	0·66	0·66	0·66	0·66
9	0·83	0·66	0·66	0·66
10	0·66	0·66	0·66	0·66

Directly after the test on March 16th the joints were removed from the water and exposed to the atmosphere until the 5th inst. when they were tested in water under precisely the same conditions, and the result was identically the same as the third test in the above list. It will be understood that this system of jointing is not applicable to submarine cables unless in cases of emergency, but is all that could be desired for subterranean and other wires. A box with all materials necessary for making one hundred joints in ordinary No. 7 wire, and samples of the complete joint, are placed on the table for inspection.

WILLOUGHBY SMITH.

Wharf Road, City Road,
May 8th, 1876.

EXPERIMENTAL JOINTS RETESTED.

No.	Made.	Material.	Accumulation after one minute; 100 cells.
4	Jan. 13, 1876	Vulcanite	5°
7	„	„	5°
8	„	„	4°
9	„	„	4°
13	„	Wood	5°
11	Feb. 2, 1876	„	5°
15	Feb. 12, 1876	„	5°
16	„	Vulcanite	4°
17	„	Wood	4°
18	„	Vulcanite	5°
19	„	„	4°
20	„	Wood	4°
18s No. 7	Same length	—	3°

July 4th, 1876. W. S.

NOTES ON A THUNDERSTORM WHICH PASSED OVER CLEVEDON, ABOUT 5 P.M. ON MARCH 15TH, 1876.

There was but a single flash, which appeared to many observers to travel horizontally through the air. However the lightning passed down the lightning-conductor of Christchurch. The flag-staff, about 100 feet high, and the four pinnacles, about 90 feet high, have each a conductor, the flag-staff having the usual conical point, the pinnacles having the copper rope attached to their vanes. The five copper ropes unite inside the tower in the neighbourhood of the clock. Lower down the conductor passes through a slanting hole to the outside, and for the lowest 12 feet is encased in a pipe. On reaching the ground it passes into a dry freestone channel for about

a dozen feet and then dips down into the drain which carries rain-water from the roof. As no rain preceded or accompanied the flash it may be presumed that the drain was dry.

The protector is copper throughout, and, with the exception of the termination, seems to have been carefully and efficiently placed. Diameter I estimate to be half inch, it may be a trifle more.

Just at the point where it leaves the pipe and enters the ground the electric charge left it, dashed through three feet or more of solid wall supporting the tower, in order to reach the gas meter inside, then it passed safely along the gas-pipe.

The cavity made was considerable, but very irregular. I was unable to ascertain when the workmen were engaged in repairs, and therefore cannot give their estimate of the weight of stone displaced, but it must have been many hundred-weights, though only a few pounds were actually thrown out on to the path, or inside into the vault. A large quantity of stone was pulverized, and the whole gave one the idea of the explosion of a charge of gunpowder under great compression.

In a house, about 100 yards from the church, the inmates felt the shock intensely, but did not know that the house had been touched. Some hours after however, on going to turn on the gas, a hissing noise was heard, and a hole was found in the composition gas-pipe, about five-eighth inch diameter, just where the pipe passed within an inch of a water pipe. The lightning must have come along the main from the church gas-pipe to this house, and then passed to the water pipe as the readiest way to moist earth. The whole soil in the neighbourhood is mountain limestone, very dry. There is not the slightest evidence of displaced plaster, or any other sign of the passage of an electrical discharge through the house.

It is difficult to say in what direction the storm was travelling, as there was but the one flash. If I can answer or obtain an answer to other questions I shall be pleased to do so.

EUSTACE BUTTON.

Lewesfell, Clevedon,
July 18, 1876.

THE PERSIAN GOVERNMENT TELEGRAPHS.

By A. Houtum Schindler,
Inspector-General of Persian Telegraphs.

The first telegraph line was erected in Persia in 1859 from Teheran to Sultaníeh, a place of little importance, about 160 miles from Teheran, on the road to Tauris. The conductor consisted of three kinds of wire—a thin copper wire, a thin iron one, made at Ispahan, and a galvanised iron wire of 2·50 millimètres diameter, bought in Russia—and was insulated with small earthenware insulators of a cylindrical shape, made at Teheran; the wire was wound round each insulator. The instruments were Breguet's alphabetical dials. This line worked for only one summer; the Shah left Sultaníeh in the autumn, and the line, being of no further use, was demolished.

A line from Tauris to Teheran was erected in the following year (1860). Half of it was 4 millimètres wire, the other half was less than 2·50 millimètres. The insulators were made at Teheran, of a sort of porcelain dust and earthenware mixed, and were of a curious shape. Seen from the side they resembled the letter J; a nail at the top fastened them to the post, and the wire was suspended by the lower part. These insulators were found useless, and were next year replaced by some bought in Europe. The line worked at intervals till 1869, when it was replaced by Siemens's line.

In 1863 the Teheran-Tauris line was continued to the Russian frontier on the Arras River by a line of 3 millimètres diameter, insulated by French porcelain insulators. In this year embossing Morse instruments were bought from Försterling, of St. Petersburg.

In 1863 the English Government also began to erect a line, of No. 5 galvanised iron wire (B.W.G.) on wooden poles, and Siemens's iron-hooded insulators, from Bushire to Teheran, and thence to Khanegín on the Turkish frontier, on the road to Bagdad. This line worked through from Bushire to Bagdad on the 13th October, 1864.

In this same year two other lines were completed by the Persian

Government. One went from Gásvín to Resht, and was constructed of all sorts of wires and insulators, the remains of the former lines; it worked for two years, was then on the ground for two years, and was repaired or rather re-erected in 1869 with No. 5, B.W.G., wire and Russian double-bell porcelain insulators. It works now. The other line joined Teheran with Astrábád, *viâ* Fírúzkúh, Sárí, and Barfurúsh. It was at first well insulated by French insulators, had straining-posts with ratchets, and 3 millimètres wire. As it was not kept in repair it was soon for the greater part on the ground, and not long after deserted.

In 1869 a second line was commenced between Teheran and the Russian frontier; it was No. 5, B.W.G., wire, insulated by Russian double-bell porcelain insulators. The arrival of Mr. Siemens in this country put a stop to the works, which had progressed for a distance of eighty-five miles.

The English Government had drawn a second wire from Bushire to Teheran and Kermanshah in 1867, but gave up the line from Teheran to Kermanshah and Khanegín in the early part of 1870. The Persian Government rolled up one of the wires from Teheran to Kermanshah and used it for other lines.

By autumn 1870 the Persian Government finished a line of No. 5, B.W.G., wire, on wooden poles, and Russian double-bell insulators, from Teheran to Astrábád, *viâ* Sháhrúd. It works well at present.

Siemens's line from Teheran to Tauris and the Russian frontier was opened for traffic on the 31st January, 1870. It consists of three wires of No. 5, B.W.G., supported on iron poles, and insulated by Siemens's iron-hooded insulators.

In 1872 a branch line, joining the important town of Khoi with Tauris, was constructed. The line went from Khoi to Merend, on the Indo-European Telegraph Company's line, between Tauris and the Russian frontier, and was No. 5 wire and Russian double-bell insulators. It works now.

In 1873 branches were erected joining Dowletabad, the principal place of the Maláyer district, Sultánábád, the principal place of the Irág district, and Burújird, with each other and with Hamadan. These branches are working well.

The English Government changed its wooden poles for iron posts, and finished a third wire from Bushire to Teheran in 1874.

The telegraph lines which are at present existing in Persia are as follows :—

	LINES.	Length in English miles.	Date of Completion.
1	Teheran to Bushire (English Government line of three wires; one wire is reserved for Persian traffic) . .	735	1864
2	Teheran to Khanegín (constructed by English Government: handed over to Persian Government 1870) . .	440	1864
3	Teheran to Summer Palaces of H. I. M. the Shah	18	1865—67
4	Gázvín to Resht	105	1869
5	Teheran to Julfa (Russian frontier); (constructed by Siemens; three wires, one reserved for Persian traffic) .	415	1870
6	Teheran to Sháhrúd and Astrabad .	314	1870
7	Merend to Khoi	35	1872
8	Hamadan to Dowletabad, Burújird, Sultánábád	120	1873
	Total length of lines . .	2,182	

There are 2,182 miles of wire worked by the Persian Government, 1,470 miles by the English Government, and 830 miles by the Indo-European Telegraph Company, making a grand total of 4,482 miles of wire worked in Persia. The distances are only approximate: real through measurements have hardly ever been taken in this country.

The Persian Telegraphs are held by the Director-General for a certain sum, which he annually pays to the Shah, and he works them at his own risk, profit, or loss.

The expenditure for the year of the Hejreh 1291, that is, from 17th February, 1874, to 6th February, 1875, amounted to 358,510

francs, including all salaries, maintenance, and repairs of lines, purchase of necessary stores, &c. For the year 1292 (6th February, 1875, to 27th January, 1876) the expenditure amounts to 450,000 francs, as 375 miles 4 millimètres wire, a few thousand small Russian double-bell insulators, and 1,000 Siemens's insulators for iron-posts have been bought. The wire will be used for the line from Shahrúd to Meshhed, 296 miles, now in course of construction, and for connecting some small outlying stations with the main lines. The lines to be constructed after the Meshhed line are the Astrábád-Sárí, the Ispahan-Yezd-Kerman, a line to Kurdistan, and some smaller ones.

Excepting a small part near the Turkish frontier all the lines are on wooden poles. For the purpose the Oriental plane, the poplar, and the mountain cypress are employed. The latter is the best wood we have for telegraph poles: it grows in Gílán and Majenderán, and can easily be got in straight poles of five to six mètres in length. It is called "Avers," or "Sarv i Kúhi," *i.e.* mountain cypress. Our wire is for the greater part No. 5, B.W.G. (5·5 millimètres); a large part of the lines consists of No. 8 wire, and a very small part of No. 11. Of insulators we have Siemens's iron-hooded, Russian double-bell, large and small, and a few old French insulators; we also use many insulators of different shapes, made at Teheran, of porcelain bells or cylinders with iron hooks. Some of these latter answered very well indeed; we place them now generally in the plains, and use the stronger and better European insulators for mountainous parts of the country. This year we have made a new kind of insulator: it consists of an iron semicircle, to which is welded an upright iron hook, holding a porcelain double-bell. The semicircle is fastened to the pole by two stout nails, and can be bent so as to fit any sized pole. The bell is fastened to the hook by a cement made of gypsum, to which a very small quantity of glue has been added.

The following table gives the number of offices, instruments, and the staff of the Persian telegraphs. The offices are open all *day* only, during the nights they are closed. The instruments are mostly Morse, with relay made by Siemens. Some are without clockwork: a few of the old Russian Morse instruments also exist.

The batteries in use are principally Daniell's; we have also a few Meidinger's and Minotto's. The higher grade signallers also do duty as inspectors.

TABLE showing Offices, Staff, Instruments, Batteries, of the Persian Government Telegraph, 1st Muharrem, 1293 (28th January, 1876).

		Chefs de bureau.	Signallers and Inspectors.	Writers.	Supernumeraries and Pupils.	Line guards.	Messengers.	Instruments.		Battery Cells.
								In use.	Reserve.	
1	Abádeh	...	1	...	...	[1]...	1	1	...	24
2	Aivan i Keif	...	1	...	...	2	1	1	...	24
3	Astrabad	1	2	...	...	5	2	1	...	48
4	Burujird	1	2	1	...	1	1	1	...	48
5	Bushire	1	1	...	...	...	1	1	...	48
6	Dámghán	...	1	...	...	3	1	1	...	48
7	Hamadán	1	3	1	...	5	4	3	...	52
8	Ispahan	1	3	1	...	...	2	2	...	92
9	Julfa	...	1	...	...	...	1	1	...	24
10	Káshán	1	1	1	[2]1	...	2	1	...	24
11	Kázrún	...	1	...	...	...	1	1	...	24
12	Kengáwer	...	2	...	...	2	1	1	...	24
13	Kerind	...	1	...	...	2	1	1	...	24
14	Kermánsháh	1	2	1	...	2	2	2	...	48
15	Khánábád	...	1	...	...	2	1	1	...	24
16	Khánegin	...	1	...	...	1	1	1	...	24
17	Khoi	...	1	1	...	1	1	1	...	48
18	Khwár	...	1	...	1	2	1	1	...	24
19	Maláyer	1	1	1	...	3	1	3	...	48
20	Manjíl	...	1	...	...	4	1	1	...	24
21	Merend	...	1	...	...	1	1	1	...	24
22	Miáneh	...	1	...	...	...	1	1	...	24
23	Nobarán	...	1	...	...	3	1	1	...	24
24	Qasr i Shirón	...	1	...	...	2	1	1	...	24
25	Qázvín	1	3	1	...	2	2	2	...	48
26	Qum	...	2	...	1	...	1	1	...	24
27	Qumísheh	...	1	...	...	...	1	1	...	24
28	Resht	1	3	1	...	5	2	1	...	48
29	Semnán	1	2	...	1	2	1	1	...	48
30	Shahabdulazím	...	1	...	...	...	1	1	...	24
31	Sháhrúd	1	2	...	...	3	1	1	...	48
32	Shíráz	1	3	2	...	...	5	2	...	92
33	Sultánábád	...	1	...	...	2	1	1	...	48
34	Tabríz	1	8	2	...	...	6	3	...	92
35	Teherán	1	9	12	25	9	13	7	14	200
36	Zenján	1	1	1	...	...	1	2	...	52
37	Doshantepeh [3]	...	...	...	...	...	...	1	...	48
38	Sultanetabad [3]	...	...	...	...	...	...	1	...	48
39	Palace in town [3]	...	...	...	...	...	...	1	...	48
	Totals[4]	16	68	26	29	64	66	56	14	1722

Remarks.

[1] Stations on the English lines have no gholams, as the maintenance is in the hands of the English Government and of Siemens.

[2] Supernumeraries do regular office duty but receive no salary.

[3] *These three stations are open only during the stay there of the Court, and are worked by one clerk, who ranks as a superintendent, or chef de bureau.

[4]
Chefs de bureau	16
Signallers and inspectors	68
Writers	26
Supernumeraries	29
Line guards	64
Messengers	66
Total	269

Besides the staff enumerated in the table there are:

Director-General	1
Inspector-General	1
Controllers	2
Sub-Director	1
Inspectors and Superintendents	11
Treasurer	1
Mechanician	1
Total	18

which, added to the total number of the staff shown in the table, gives a grand total of 287.

We have a number of signallers who can work the *three* different alphabets in use in our department of Morse adapted to Persian language, the Morse as used in European administrations, and the Morse as used in Turkey, that is *five different* alphabets in all, with equal facility. Thirty words, of a length of five letters, per minute is about the average speed.

Regarding the number of messages sent and received no proper registers exist at present. All Government messages are gratis and no register is kept of them; service messages and other free messages are also not registered. Only the paid messages are counted, and they amount for the year to somewhat over 175,000 ten-word rates. The Government messages for last year I estimate at about 400,000 ten-word rates, and the service and other free messages to about 100,000 rates, making altogether a grand total of 675,000 rates transmitted in the interior. The rates for messages in the interior are 2·50 francs for the first ten words, and 2 francs for every additional ten words or part of ten words.

The average number of international messages terminating and originating in Persia is 160 per month; the international messages in transit are counted in the returns of the Indo-European Telegraph.

I may add, that the erection of the line from Sháhrúd to Meshed has been commenced, and will probably be finished before the autumn.

LINE SHAHRUD TO MESHED.

The construction of this line was begun on the 23rd May and completed on the 31st July, 1876. The length of the line is 271 miles. It was opened for traffic on the 3rd August. The wire is galvanized iron wire of 4 millimètres diameter (No. 9, B.W.G.) The poles are poplar and fir. The insulators are double porcelain bell, with screw or with hook and nails. The line has eight stations, viz. Rahmetabad (opened during the winter only), Meiomey, Miandasht, Abbasabad, Mazinân, Sabzvar, Nishapúr, and Meshted. At Sabzvar are translating instruments. The instruments are Siemens's Morse with polarized relays, but without writer and clockwork, work being done by sound.

The workmen suffered much from want of water and from heat. During the months of June and July the heat in the plains, with a cool wind blowing, rose to 140 and 141 degrees Fahrenheit, while the heat in the shade once rose to 112 degrees Fahrenheit. Great anxiety was felt on account of the Turcomans, who were expected to attack us every day while we were in the four stages known as the "stages of terror" (Shahrúd to Mazinân), but not a single Turcoman was seen.

ABSTRACTS AND EXTRACTS.

Read before the INSTITUTION OF CIVIL ENGINEERS, *Feb.* 16, 1858, *and reprinted from their Proceedings by the kind permission of that body.*

ON SUBMERGING TELEGRAPHIC CABLES.

By JAMES ATKINSON LONGRIDGE, M. Inst. C.E., and CHARLES HENRY BROOKS.

The failure in the first attempt to lay the Atlantic cable has, doubtless, attracted the attention of many Members of this Institution to the subject of submerging such lines of telegraphic communication; and in the hope that the following attempt to investigate the laws to which such operations are subject will not be unacceptable it is now submitted by the Authors to their professional brethren—not however claiming to be more than a partial solution of an interesting and somewhat complicated problem.

Those upon whom the task devolved of laying the cable between Ireland and America have, without doubt, given the whole question careful consideration, and the high attainments of some of them, in mathematical and physical science, lead to the belief, that by them, at any rate, the conditions of the problem have been appreciated. At the same time it cannot be denied that much misapprehension does exist, and manifests itself by the various schemes which are, from time to time, proposed to prevent another failure.

Some discussion arose at the meeting of the British Association, at Dublin, in 1857, after the reading of one or more papers before the Mechanical Section; but the published reports were meagre and unsatisfactory, and one of the reported conclusions appeared

to the Authors of this paper to be so curious and improbable that their attention was, independently of each other, attracted to the subject. The conclusion just adverted to was thus expressed in some of the Journals:—"During the conversation which arose in the Section, after the reading of this communication, a new light seemed to break upon the members, as it seemed to be universally admitted that it was mathematically impossible, unless the speed of the vessel, from which the cable was payed out, could be almost infinitely increased, to lay out a cable in deep water (say two miles or more), in such a way as not to require a length much greater than that of the actual distance, as from the inclined direction of the yet sinking part of the cable the successive portions payed out must, when they reached the bottom, arrange themselves in wavy folds, since the actual length is greater than the entire horizontal distance." It seemed desirable to ascertain how far such an idea as that involved in the above statement was correct, and, if correct, what amount of slack ought to be provided to meet the waste in varying depths of water. This was the primary question which the Authors of this Paper proposed to themselves, and upon which they were each independently engaged. They subsequently communicated their ideas to each other. The inquiry branched off in other directions, and the results are now jointly submitted to the consideration of the Members of the Institution.

The following are the questions which are discussed, generally, in the body of the Paper, the calculations being, for the most part, given in an Appendix:—

I. Is it possible to lay a cable straight along the bottom, in deep water, free from the action of currents?

II. If possible, what degree of tension is required, in paying out, so as to lay the cable straight?

III. What is the effect on the cable, as regards strain, of varying

(*a.*) The depth of the water,

(*b.*) The specific gravity of the cable, and

(*c.*) The velocity of the paying-out vessel?

IV. What are the relative velocities of the cable and of the paying-out vessel, requisite to reduce the strain, or tension, to any given amount, and what will be the consequent waste of cable?

V. What is the effect of currents and the consequent waste of cable?

VI. How far is it necessary and safe to check the velocity of paying out, in passing currents, so as to avoid, as far as possible, waste of cable?

VII. Is it safe, and, if so, under what circumstances, to stop the paying out, and to attempt to haul in the cable from great depths?

VIII. What is the effect of the vessel pitching in a heavy sea?

IX. What are the desiderata in the paying-out apparatus?

X. What would be the effect of floats or resisters?

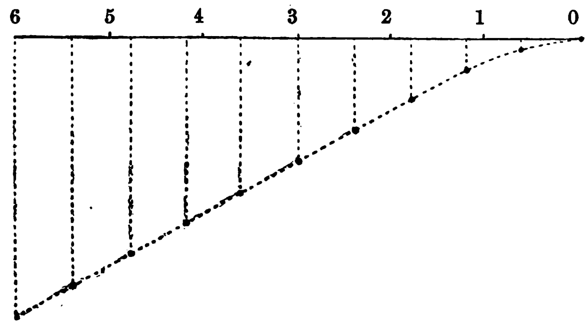
XI. What are the best means for saving the cable in case of fracture?

XII. What is the best mechanical construction of a submarine telegraphic cable?

I.—Is it possible to lay a Cable straight along the bottom, in Deep Water, free from the Action of Currents?

If a vessel moving uniformly forward drop, at equal intervals of time, balls of equal size, and of the same material, it may be shown, that in water, and with a material of the specific gravity of the Atlantic cable, the motion of each ball vertically will soon become uniform, and the line drawn through the whole of the

Fig. 1.



balls, at any instant of time, will be very nearly a straight line, descending obliquely from the ship to the bottom. Fig. 1 shows

the form of the line drawn through such a series of balls, dropped from a vessel moving at the rate of 6 feet per second. The equations of motion are (Appendix, Problem I.)—

$$v = \sqrt{n^2 - (n^2 - \nu^2)\,\epsilon^{-2cx}} \quad \ldots \ldots \ldots \ldots \quad (a).$$

$$t = \frac{1}{2\,c\,n} \log \frac{n + \sqrt{n^2 - (n^2 - \nu^2)\,\epsilon^{-2cx}}}{n - \sqrt{n^2 - (n^2 - \nu^2)\,\epsilon^{-2cx}}} \cdot \frac{n - \nu}{n + \nu} \quad \ldots \quad (b).$$

which, when x is large, becomes, very nearly,

$$v = n \quad \ldots \ldots \ldots \ldots \quad (c).$$

$$t = \frac{x}{n} + \frac{1}{cn} \log \frac{2\,n}{n + \nu} \quad \ldots \ldots \ldots \ldots \quad (d).$$

The velocity, consequently, ultimately becomes n, which is a quantity depending on the form and specific gravity of the sinking body. Although this ultimate velocity is only attained at an infinite depth, yet it is rapidly approached from the beginning of the motion. If a sphere of the same diameter and specific gravity as the Atlantic cable is placed in water, and is allowed to descend, it can be shown that in six-tenths of a second it will have acquired a velocity of 3·27766 feet per second, only differing from its ultimate velocity by one three-thousandth part. The depth passed through, in acquiring this velocity, is not quite 2 feet, so that in dealing with considerable depths of water the terminal velocity may be assumed as the true velocity throughout.

From these equations the time of descent of a sphere of the specific gravity and diameter of the Atlantic cable, through 2,000 fathoms of water, is found to be 48 minutes; and if the ship is moving at the velocity of 6 feet per second, it would have moved forward 17,449 feet whilst the body sank to the bottom; consequently the length of the imaginary line passing through the series of balls would be 21,177 feet. But it is evident that each ball would sink vertically. If then each particle of a cable followed the same law, and no tension was applied at the top, there would be 21,177 fathoms of cable paid out for each 17,449 fathoms run by the ship, or a loss of cable of about 21 per cent. It will be readily seen that this loss would increase with any increase in the

specific gravity of the cable, and *vice versâ*. A continuous cable would not, however, sink in the same way as a series of unconnected balls. If a rod, of any material heavier than water, is placed in water in an inclined position, it will be found, on letting it go, that it does not sink vertically, but moves diagonally downwards to the lower side. In fact, it runs down an inclined plane, lying between the vertical and its own direction. The equations of motion, under these circumstances, of a body, such as the cable, descending obliquely without tension, are (Appendix, Problem II.)—

$$v = \sqrt{w\left(\frac{\cos a}{q'} + \frac{\sin a}{q}\right)} \dots\dots\dots (e).$$

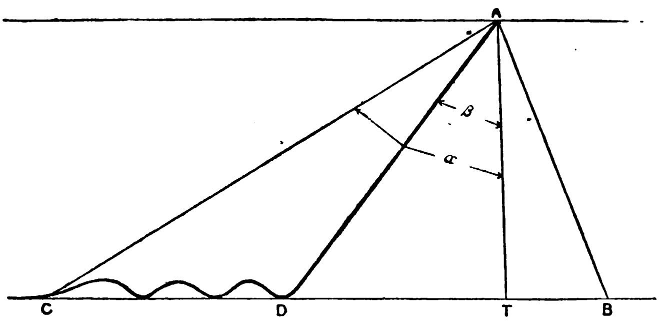
$$\tan (a - \beta) = \sqrt{\frac{q'}{q}\tan a},$$

or $$\beta = a - \tan^{-1}\sqrt{\frac{q'}{q}\tan a} \dots\dots\dots (f).$$

β being the angle of the direction of motion, and a the angle of the cable with the vertical.

If the cable runs out free from tension at the top, these equations give the circumstances of its descent, and by calculating the angle β (fig. 2), the distance which each particle would run before it reached the bottom may easily be found.

Fig. 2.



The particle at A instead of coming to B, so as to make CB= AC, would arrive at D, so that the whole length AC would be

deposited in folds, or coils, between C and D, and the waste of cable would be the difference between AC and CD divided by AC. The formulæ for calculating the angle β and the waste of cable, are given in the Appendix, Problem II., and by means of them the following tables have been calculated for two descriptions of cable. The first is the Atlantic cable, with a specific gravity of 3·489; the other is a lighter cable, with a specific gravity of 1·50:—

ATLANTIC CABLE.

Velocity of the paying-out vessel, in feet per second	0	2	4	6	8	10	12	15
Inclination of the cable to the horizon . . .	90°	68° 37′	41° 44′	28° 45′	21° 47′	17° 31′	14° 38′	11° 44′
Angle of motion with, or inclination of line of descent of each particle to, the vertical . . .	0	16 50	40 35	51 30	56 49	59 43	62 22	62 39
Velocity of the cable, in feet per second . . .	24·201	24·13	22·7	22·06	22·15	22·82	23·77	25·60
Waste per cent. of cable payed out	100	92	83	73	64	56	50	41

THE LIGHT CABLE.

Velocity of the paying-out vessel, in feet per second	0	...	4	6	8	10	12	15
Inclination of the cable to the horizon . . .	90°	°...′	19° 56′	13° 21′	10° 2′	8° 2′	6° 43′	5° 22′
Angle of motion with, or inclination of line of descent of each particle to, the vertical .	0	...	58 7	62 00	63 7	63 14	62 55	62 4
Velocity of the cable, in feet per second . . .	11·024	...	10·20	9·83	12·48	14·02	15·70	18·31
Waste per cent. of cable payed out	100	...	61	39	36	29	24	18

It has been proposed that, in order to lay a cable safely in a considerable depth of water, it should be suffered to run out freely from a drum without tension, whilst the paying-out vessel should be kept at the highest possible speed. An inspection of the above tables shows with what a waste of cable such a proceeding would be attended; for, with the Atlantic cable, and a velocity of the paying-out vessel of 15 feet per second, or about 10 miles per

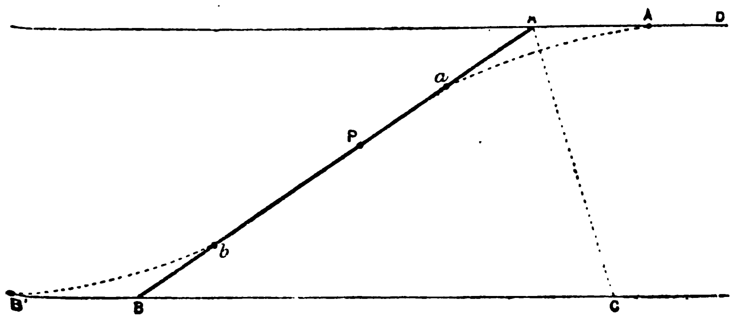
hour, the waste of cable would be 41 per cent. With a light cable it would certainly be less; but even with the above-mentioned cable, having a specific gravity of 1·5, it would be 18 per cent. at this high speed of the ship. The above formulæ also give the velocity of sinking of the cable, in a vertical and horizontal position, in feet per minute, to be as follows:—

	Vertical.	Horizontal.
Atlantic cable	24·201	3·082
Light cable	11·024	1·404

Lastly, they give the direction of motion and velocity of the end of the cable in case of fracture, and so may be useful in estimating any proposed means of catching it under such circumstances.

The waste consequent on the tendency to run backwards may be prevented by paying out the cable under a certain amount of tension. This naturally leads to an extension of the same idea, and gives rise to the inquiry before proposed, whether it be possible so to adapt the tension that a forward motion may be given to each particle sufficient to cause the cable to lay in a straight line along the bottom, free from tension on the one hand and from bends and coils on the other.

Fig. 3.



If the cable assumes the form of a straight line A B (fig. 3), it is evident that to bring it in order to lie straight at the bottom the point A must move in the direction A C, bisecting the angle B A D, and that each point between A and B must move parallel

to A C; that is to say, the motion of every point in the cable must be in a direction bisecting the angle formed by the cable and the horizon, and this condition may easily be shown to be necessary whatever be the form taken by the cable. (Appendix, Problem III.)

In the case of a straight line all that would be necessary to ascertain the tension, would be find what force would bring an inflexible rod A B, into the position B C, against the resistance of the water. But there is no reason *à priori* for concluding that the line of the cable is straight. Indeed, a little consideration leads to an opposite conclusion; for if A B is the direction of the cable sinking without tension, and a force is applied at A, in order to compel A to move forward so as to arrive at C whilst it sinks to the bottom, it is to be expected that the top part of the cable will be drawn forward, and assume some new position, such as A′ *a*. Again, the tension in A B, unless it vanishes at B, will prevent the existence of an angle at that point, and another curve B′ *b* will result. It is, therefore, necessary to ascertain the form of the curve assumed by the cable under tension, in order to estimate the resistance of the water to its motion; and as this depends at any point upon the inclination of the cable to the horizon at that point, the problem becomes somewhat complicated. As it is not yet supposed to be known whether it is possible to lay the cable straight without some amount of tension at the bottom, it is necessary to frame the equations on the supposition of such a tension existing, and by making it = 0 in the final result the effect will then be known. The problem to be solved, then, is to find the equation to the curve A′ B′. The simplest mode of proceeding seems to be to consider the cable and the ship at rest, and the water moving, not horizontally, nor in any one direction, but in such a manner that its direction at any point P bisects the angle made at P by the cable and the horizon, and with a velocity equal to the actual velocity of the point P in that same direction.

This mode of considering the question enables it to be treated statically as regards the form of the curve; and the following are the forces:—

1. Its own weight in water, acting vertically.

2. The tension of the vessel, acting in the direction of the curve at the surface of the water.
3. The tension at the bottom, acting horizontally in a direction opposite the motion of the vessel.
4. The resistance of the water, acting at every point of the cable, in a direction bisecting the angle formed at that point by the cable and the horizon.
5. The friction of the water on the cable.

By resolving these forces into vertical and horizontal components, differential equations are obtained, and from them is deduced the following expressions:—

$$\frac{1}{w - m'\dfrac{(p - \sqrt{1+p^2})^2}{\sqrt{1+p^2}}} \log \frac{w\,a}{w\,a + \left\{w - m'\dfrac{(p-\sqrt{1+p^2})^2}{\sqrt{1+p^2}}\right\}x}$$

$$= \frac{1}{\sqrt{w^2 + 4m^2}} \log \left\{\frac{2\,m\sqrt{1+\dfrac{1}{p^2}} - w - \sqrt{w^2 + 4\,m^2}}{2\,m\sqrt{1+\dfrac{1}{p^2}} - w + \sqrt{w^2 + 4\,m^2}} \cdot \frac{2m - w + \sqrt{w^2 + 4m^2}}{2m - w - \sqrt{w^2 + 4m^2}}\right\}.$$

This, it will be observed, is not an equation between x and y, but between x and $p = \frac{dy}{dx}$, or the cotangent of the angle formed by the curve and the ordinate corresponding to the abscissa x; but from it the form of the curve may be found, without further integration, which would be required to obtain the ordinate in terms of the abscissa, and would be very complicated, if not impracticable. It is shown, in the Appendix, that the effect of increasing the friction of the cable is to diminish the radius of curvature near the bottom. In the case of friction being disregarded, the equation above given has been integrated, and x obtained in terms of y. It is further shown, that if the tension at the bottom be = zero, the cable takes the form of a straight line.

Having thus shown that it is quite possible to lay the cable straight along the bottom, the Authors proceed to investigate the amount of the tension, which forms the second head of the inquiry.

II. What degree of tension is required in paying out, so as to lay the Cable straight?

The general differential equation of the tension cannot be exactly integrated; but the supposition upon which the integration has been effected is one that will not materially influence the result, and in fact becomes strictly true, if the tension at the bottom be $= 0$.

The equation is—

$$t = w\,(x + a) - m' \frac{(1 - \cos A)^2}{\sin A}\, x$$

It shows that the tension at any point is equal to the weight in water of a length of cable equal to the depth of the water below that point, plus the tension at the bottom, less an amount due to the friction of the water against the cable.

The next heads of the inquiry are:—

III. What is the effect on the Cable, as regards strain, of varying?

(*a*.) The depth of the water,
(*b*.) The specific gravity of the cable, and
(*c*.) The velocity of the paying-out vessel?

IV. What are the relative velocities of the Cable and of the paying-out Vessel, requisite to reduce the strain, or tension, to any given amount, and what will be the consequent waste of Cable?

These questions being connected in their nature, are considered together.

It is evident, from the above equation, that the tension increases uniformly as the depth, and as the weight of the cable in water;

from which it follows that the less the specific gravity of the cable the less is the risk from overstrain. It is further apparent that the strain is diminished by any increase of the co-efficient (m') of friction, and it is therefore a subject for inquiry how far this can be practically accomplished. This question comes properly under the tenth head of the inquiry, and it is, therefore, laid aside for the present.

Another method of diminishing the tension is by increasing the velocity of paying-out beyond that of the paying-out vessel. The effect of this is investigated in the Appendix, where it is shown that if

v be the velocity of the cable,
ν that of the ship, the tension

$$t' = w\,x - \frac{m'\left(\frac{v}{\nu} - \cos \mathrm{A}\right)^2 x}{\sin \mathrm{A}}$$

from which the values of t' may be found for any values of x, v, and ν. These values have been calculated for two cables, viz. the Atlantic, and one of a specific gravity of 1·50, and tables are given, showing for each the decrease of tension attendant upon increased ratios of the speed of the cable to that of the ship. These results are exhibited in figs. 4 and 5. In fig. 4 the vertical column on the left shows the tension of the cable in lbs., the numbers at the top show the rate of the paying-out vessel, and the ratio at the end of each curved line is that which the velocity of the cable bears to that of the vessel. The upper series of curves refer to the Atlantic cable, and the lower series to the light one. In fig. 5, which refers to the Atlantic cable only, the number at the end of each curve is the rate of the vessel in feet per second; the ratio at the top is that of the velocity of the cable to that of the vessel, and the numbers at the left side show the corresponding tension. It is found that the diminution of tension due to an increased rate of paying out is comparatively small, unless the velocity of the ship itself is considerable. In fact, the decrease of tension, arising from letting the Atlantic cable run out at twice the speed of the ship, is when the—

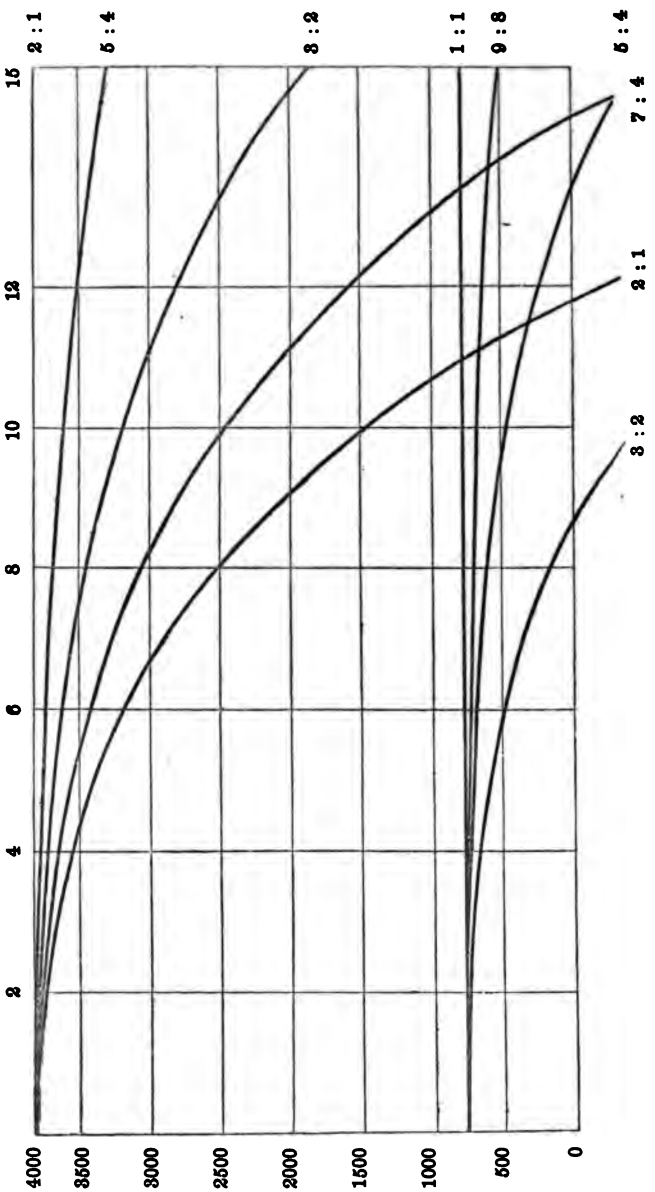


Fig. 4.

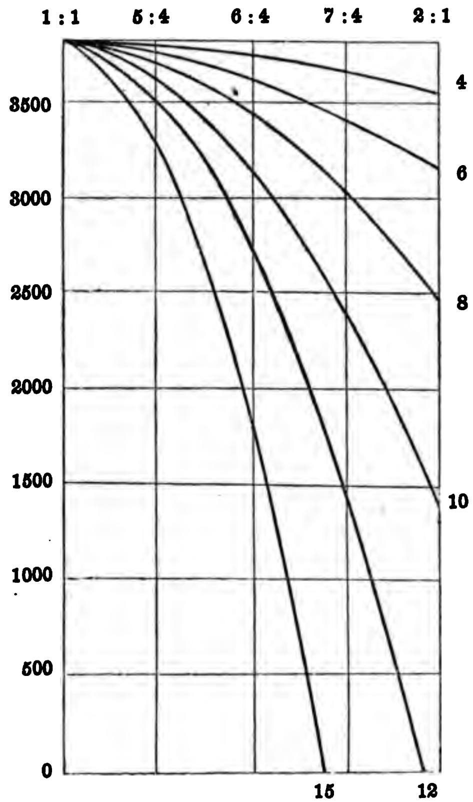
Speed of the ship is		4 feet per second,		251 lbs.	=	$6\frac{1}{2}$ per cen	
"	"	6	"	622 "	=	16	"
"	"	8	"	1,301 "	=	34	"
"	"	10	,	2,393 "	=	62	"

and at 12 feet per second the tension would be nil.

Again, if the speed of the cable is only one-fourth greater than at of the ship, the decrease of tension is, when the

Speed of the ship is	4	feet per second,	32 lbs.	= 0·8	per cent.
,, ,,	6	,,	69 ,,	= 1·8	,,
,, ,,	8	,,	117 ,,	= 3·0	,,
,, ,,	10	,,	192 ,,	= 5·0	,,
,, ,,	12	,,	299 ,,	= 7·8	,,

Fig. 5.



Taking the light cable, it appears that if the velocity of the cable is one-fourth greater than that of the ship, the decrease of tension is, when the

Speed of the ship is	4	feet per second,	30 lbs.	=	3·7	per cent.
,, ,,	6	,,	78 ,,	=	9·8	,,
,, ,,	8	,,	166 ,,	=	20·8	,,
,, ,,	10	,,	318 ,,	=	39·9	,,
,, ,,	12	,,	546 ,,	=	63·9	,,

From this it appears that the relief obtained by this method of decreasing the tension is, at the ordinary velocity of paying out, very inconsiderable; whilst the waste of cable is very great. It is submitted that the true remedy for the evil of great tension is the employment of a cable of small specific gravity. If, for instance, the two cables above mentioned are taken, it is seen that there is a remarkable difference in the tension, the light cable having a tension, in 2,000 fathoms, of 879 lbs., against 3,849 lbs. in the case of the Atlantic cable. It is further to be observed, that so long as the velocity of paying out does not exceed that of the ship, no advantage is derived from increasing the speed of the ship, but that, on the contrary, a slight increase of tension must result. The great waste of cable attendant on a slight deficiency of tension, as indicated by these tables, seems to point to the desirability of laying cables with some moderate amount of tension at the bottom; because it is evident that a very moderate increase in the depth of the water would be attended with a great waste of cable. If, for instance, the ship was moving with a velocity of 6 feet per second, the tension on the cable, at a depth of 2,000 fathoms, would be 3,849 lbs. If, now, the depth is increased by 100 fathoms, the increase of tension due to this depth would be about 190 lbs. In order to balance this, the same extra resistance must either be applied by the breaks, or the cable must run out at a velocity one-half greater than that of the ship, or at 9 feet per second, and consequently with a waste of 33 per cent. In the case of the light cable, a similar increase of depth would, if not resisted by the breaks, give rise to a velocity of cable of about $7\frac{1}{5}$ feet per second, thus involving a waste of $16\frac{2}{3}$ per cent. of the cable. These tables will serve to account for the sudden increase of velocity which has been mentioned in laying heavy cables, when the depth of water has increased; and they show how desirable it is to be prepared with the fullest information

respecting the depth to be traversed, and to have in readiness efficient means, under the control of vigilant and intelligent men, so that by a proper and gradual increase of resistance by the breaks, the cable may be prevented from acquiring any undue velocity.

V. What is the effect of Currents, and the consequent Waste of Cable?

The depth to which the ocean currents extend, their breadth and their velocity, are difficult to ascertain; but the Authors enter upon this part of the subject in the hope that their investigations will be of practical advantage, in so far as they serve to point out the danger and inutility of attempting to check the running out of cable due to a current. The action of currents upon a cable increases with the length exposed at any moment of time, and as the extent of the currents is much greater horizontally than vertically it is obviously desirable that the cable should traverse them in the shortest possible direction consistent with other necessary conditions. In this respect then a heavy cable is to be preferred, as it descends at a higher angle; and it is worthy of consideration how far it may be practicable to increase the velocity of sinking by attaching weights to the cable whilst passing through a current. This, the Authors think, may be accomplished without much difficulty; but before giving any opinion upon its desirability they prefer to examine what would be the approximate loss which a current of a given extent and velocity might occasion.

Let figs. 6 and 7 represent a ground-plan and section of the ship's course, $A\ A_{,}\ A_{,,}$, fig. 6, and of the position of the cable $A'\ B'$, fig. 7, and let the commencement of the current be at X Y. When the ship moves past A' it will, unless prevented, drift with the current in some direction such as $A_{,}\ a$ (fig. 6), and the first effect will be to give the sinking cable an apparent motion in a contrary direction; but after the action of the current comes upon the whole suspended portion of the cable it will go on depositing in the altered line of the ship's course, as if no current existed.

But if the ship is kept to a true course the action will be entirely different; and it is now to be considered what would be the effect on the cable, as regards running out and tension. If the current

Fig. 6.

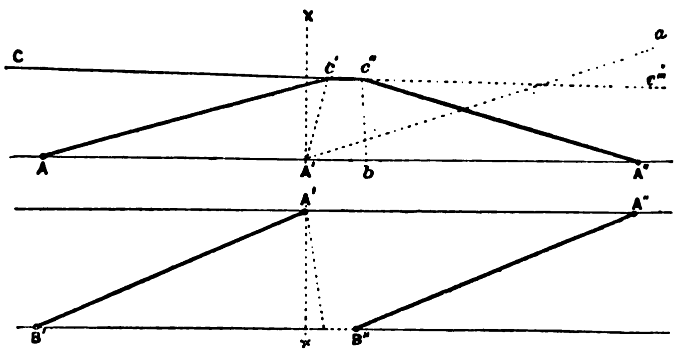


Fig. 7.

flows with such a velocity as to move a cable laterally, to a distance represented by the line c' c'' c''', whilst it sinks from A′ A″ to B′ B″, and if the lateral resistance of the water behind X Y is neglected, the cable would come into the position A c', a straight line along the bottom equal in length to A′ B′. If, on the other hand, the lateral resistance behind X Y is very great, the cable will take a direction approaching to A $A_{,}$ c'; but since at the velocity of any ordinary current the resistance would be small, and since, also, the transition from still water to current would be gradual, the line A c' may be taken as virtually a straight line. The distance to which a current would transport a particle freely suspended is that due to the velocity of the current itself. Since there is no tension at the bottom, the cable, at that depth, would be free to move laterally with the current, and the whole cable would assume a diagonal position from some point at the bottom, such as c'', to the ship at $A_{,,}$. When the ship has arrived at $A_{,,}$, the distance from $A_{,}$ due to its velocity whilst the cable is sinking, the

cable will be in the following position: A c' c'' along the bottom, and from c'' to $A_{\prime\prime}$ rising at its usual angle to the horizon, the projection of this part in the horizontal plane being represented by c'' $A_{\prime\prime}$ (fig. 6). The extra length of cable which must be paid out whilst the ship moves from A to $A_{\prime\prime}$ is shown in the Appendix, Problem V., to be given by the formula

$$\frac{d}{\nu \sin A}\left\{\nu - \sqrt{\nu^2 - v^2} + \cos A\left(\nu - \sqrt{\nu^2 - \frac{v^2}{\cos^2 A}}\right)\right\}.$$

After this there is no further waste of cable, because the suspended portion c'' $A_{\prime\prime}$ then moves on parallel to itself, and the point c'' moves forward parallel to, and at the same velocity as, the ship at A″.

In order to give some idea of the amount of waste under the action of a current, it has been calculated that for a current of 100 fathoms deep, running at right angles to the ship's course, at a velocity of $1\frac{1}{2}$ foot per second, the ship moving at the rate of 6 feet per second, the waste of the Atlantic cable would be 14 fathoms, and of light cable 28 fathoms. This shows a slight advantage, in this respect, in using the heavy cable, which descends at an angle of 28° 45′, as compared with the light cable, descending at an angle of 13° 21′. It should be observed, that practically the ultimate waste would not be quite so great, because a portion of it would be recovered on quitting the current.

The next point for investigation is the amount of strain due to the action of the current. In order to arrive at this it is shown in the Appendix, Problem VI., that the curve assumed by a flexible line stretched across a current is a catenary, but differing from the common catenary in this, that the tension is constant throughout. Now, any tension which may come upon the cable from the action of the current must follow this law, and from this fact two remarkable results proceed, viz., that the current produces no catenarian strain upon the cable and that the line from c'' to $A_{\prime\prime}$ is a straight line. For, in the first place, there is no tension at the bottom; but by the nature of the curve, if it is a curve, the tension is constant, consequently it has the same value at the top as at the bottom, where it is evidently zero. Again, if it is not a straight

line, but a curve, it must be a catenary; but it cannot be a catenary without tension at the bottom; and as there is no tension at the bottom, consequently the line is not a catenary but a straight line. That there is no tension at the bottom is one of the conditions of the problem; and it is evident that it must be so, because if the velocity of the cable exceeds, by ever so little, the velocity of the ship, the cable must be deposited in folds, or coils, and consequently without tension.

The same reasoning will apply to the case of a current whose depth is less than the total depth of the water; because each particle after passing through the current descends through the rest of its course without any further lateral deflection, beyond what is due to the portion above it in the current.

The result, that a current causes no catenarian strain on the cable, is an important one, and removes what appeared, for a long time, to the minds of the Authors, an objection to light cables, whose sole disadvantage now appears to be confined to a little extra waste on entering currents.

If, however, during the passage through a current, the paying out is stopped, or retarded, a strain will immediately arise, for the cable will then take the catenarian form from c'' to $A_{''}$. Under these circumstances the strain may be calculated, as shown in the Appendix, Problem VI., and will be additive to the vertical strain arising from the weight. A slight additional strain is brought upon a cable by a current caused by the friction of the water, but it is so slight that practically it may be disregarded. The method of estimating this is given in the Appendix, Problem VII., and the amount is shown to be quite insignificant.

VI. How far is it necessary and safe to check the Velocity of paying out in passing Currents, so as to avoid, as far as possible, Waste of Cable?

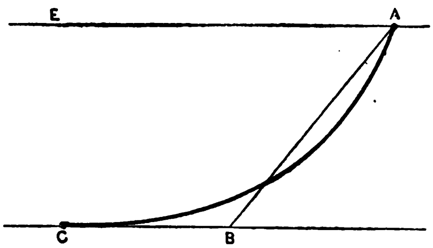
Since the waste of cable is confined to the first entrance into the current, it would seem advisable not to check it by increasing the tension, for the amount lost must, under ordinary circumstances, be very inconsiderable, even with a much lighter cable than the

Atlantic cable. For instance, if the current of the Gulf Stream is assumed to extend to a depth of 200 fathoms, which is probably beyond the truth, and to flow with a velocity of $1\frac{1}{2}$ foot per second, and if the rate of the paying-out vessel is 6 feet per second, the waste of cable, of the specific gravity of the Atlantic cable, would not be more than 28 fathoms; and this is, as shown above, independent of the width of the stream, and only occurs at the first entrance upon it. Again, in the case of a much lighter cable, having a specific gravity of 1·5, the waste would not be more than 56 fathoms, and this might be diminished, by attaching sinkers, if desired. It is therefore, maintained, that any attempt to check the running of the cable due to a current, by an increase of tension, is alike injudicious and unnecessary.

VII. Is it safe, and, if so, under what circumstances, to stop the paying out, and to attempt to haul in the Cable from great Depths?

In order to solve this question it is necessary to find the curve which the cable will assume when the paying-out is stopped, and then to calculate the tension at the vessel. It is evident, that the curve is the common catenary. Let A B (fig. 8) represent the position of the cable at any moment of time during the paying out, and let the ship and the paying out stop simultaneously. The cable which was in the position A B C will immediately begin to

Fig. 8.



rise at B, and to assume a catenarian form. It is shown in the Appendix, Problem VIII., that the following relations subsist:—

$$\frac{x \sin a}{1 - \cos x} = s - \sqrt{s^2 - x^2} + \frac{x \cos a}{1 - \cos a} \log \frac{\cos a}{1 - \sin a}$$

and

$$\sqrt{2ax + x^2} = s - \sqrt{s^2 - x^2} + a \log \frac{a + x + \sqrt{2ax + x^2}}{a}$$

when x, y, and s are the respective abscissa, ordinate, and length of the curve, a the angle formed with it and the surface, and a the tension at the bottom. Having obtained the value of a, the total tension is equal to a weight of cable of the depth x plus a.

The following tables have been calculated from the Formula 5, Problem VIII., to show the amount of strain which would be brought upon the cable by a stoppage of the paying-out apparatus, in a depth of water of 2,000 fathoms, and also the length which must be paid out, if the ship is stopped, in order to produce a minimum tension:—

ATLANTIC CABLE.

Velocity of the ship in feet per second	2	4	6	8	10	12	15
Tension in lbs. when the cable is stopped	4,704	8,624	15,736	25,760	38,050	54,460	83,100
Length to run out for minimum tension, in case of stoppage; and distance to move back, to bring the cable vertical, in feet	3,812	7,426	8,924	9,689	10,140	10,459	10,767

LIGHT CABLE.

Velocity of the ship in feet per second	...	4	6	8	10	12	15
Tension in lbs. when the cable is stopped	...	6,328	13,140	23,880	35,990	52,530	79,111
Length in run out for minimum tension, in case of stoppage; and distance to move back, to bring the cable vertical, in feet	...	9,892	10,096	15,947	11,158	11,296	11,437

From these tables it appears that the result of a stoppage of the paying-out apparatus, in a depth of 2,000 fathoms, whilst the vessel was proceeding at the rate of 6 feet per second, would be to bring the following strains on the cables:—

Atlantic cable	. . . . .	140½ cwt.
Light cable	. . . . .	117¼ cwt.

The time in which the change of form would take place would be a difficult problem to determine, but it might be considerably less than the hypothesis above stated would give, because the ship could not be stopped instantaneously, although the paying-out might be so arrested, through an accident to the apparatus. The ultimate form of the cable would be the same; but the time being less the danger would be greater. If, then, it is necessary to stop the paying-out, or if any accident should occur, involving the stoppage of the paying-out apparatus, the engines ought to be immediately reversed, and the ship be backed, as quickly as possible, until it has arrived at a position which will allow the cable to hang vertically from the stern. Again, if it is requisite to stop to repair any part of the cable, or to splice it, the ship should be put into the same position before stopping the paying-out. The distances to be moved back are given in the above tables. In attempting to haul in, the same relative position of the ship and the cable should be maintained throughout the operation, which might perhaps be accomplished by reversing the ship's course, when the cable has taken a vertical position, and then moving backwards at the same rate as the cable is hauled in. This operation cannot be regarded as otherwise than hazardous in great depths of water, on account of the practical difficulty of keeping the vessel vertically above the cable; for it is evident that any departure from that position must give rise to a catenarian strain; and any considerable amount of this, especially in a rough sea, would undoubtedly prove fatal.

VIII. What is the effect of the Vessel pitching in a Heavy Sea?

The Authors are of opinion, that if the paying-out apparatus is not too heavy, and if it works freely, no danger need be apprehended from its use. Whilst the vessel's stern is rising, the cable will be drawn out more quickly, and whilst falling, more slowly, than its ordinary rate; but no extra tension will arise, except that

due to the inertia of the paying-out drums, and of the cable upon them. The amount of this it would not be difficult to estimate, knowing the details of the apparatus and the form and velocity of the wave; but into this it is unnecessary to enter, further than to remark that it furnishes an argument for a light and free-working apparatus. It is true that the vertical rise of the ship's stern would, to some extent, call into action the catenarian strain, but only so far as the paying-out was influenced by the causes just mentioned. If the paying-out was retarded, or even entirely stopped, the only effect on the cable would be to increase the abscissa of the catenary by the amount due to half the height of the wave, an amount quite inconsiderable, in proportion to the whole catenarian strain, in such depths as from 1,000 fathoms to 2,000 fathoms.

IX. What are the Desiderata in the Paying-out Apparatus?

With reference to the paying-out apparatus the Authors would limit themselves to the expression of an opinion of what ought to be its characteristics. The principal one is, that its inertia shall be as small as possible; and this affords an argument against the views of those who advocate the use of drums, upon which the cable should be coiled. Another suggestion has been to coil the cable upon a huge turn-table; but if either of these plans were practicable it would still be liable to the objection that the inertia of the mass would be so great that the effect of the pitching of the vessel would then be felt upon the cable almost as much as if the rate of paying-out was kept strictly uniform. The only argument in favour of such plans is the prevention of kinks; but it does not appear that any difficulty arose from this cause in the paying-out of the Atlantic cable. A very important part of the paying-out apparatus is the break, and its essential characteristic should be the impossibility of any strain arising from it, under any circumstance, beyond that which is intentionally imposed. No increase of velocity should produce increase of strain, and it is only by this condition being rigidly adhered to that such operations can be conducted with safety. The Authors urge this point the more

earnestly, as they can only account for the failure of the late attempt on the hypothesis of the paying-out apparatus having in some way got out of order and a strain having arisen far beyond that recorded by the indicator. The cable was said to be running out freely, the tension indicated was under 4,000 lbs., and yet it parted, although the actual strength is given as not less than $4\frac{1}{2}$ tons. It is not stated where the cable parted, but it is the opinion of the Authors that the stoppage of the paying-out apparatus was the cause, and not the consequence, of its parting, for it has already been shown that such a stoppage would, in this instance, bring an ultimate strain of 7 tons upon the cable.

X. What would be the effect of Floats, or Resisters?

Floats, buoys, and resisters have severally been proposed as a means of diminishing the risk from tension. The action of the former would be better accomplished by using a cable of less specific gravity, because then the relief would not be partial, but would be felt at every point. It has been shown that the angle of sinking has no effect on the tension except in so far as this is modified by the friction of the water, and therefore the mere action of floats, as tending to keep the cable more horizontal, goes for little. Their only use would be to reduce, virtually, the specific gravity of the cable.

Resisters would act in relief of the tension, and be equivalent to increasing the coefficient m' in the equations. Possibly, it might be practicable so to apply them as to make the tension equal to zero; but the length to which this Paper has extended does not permit the Authors to enter further into the detail of such an arrangement.

The next question which the Authors have proposed is—

XI. What are the Best Means for saving the Cable, in case of Fracture?

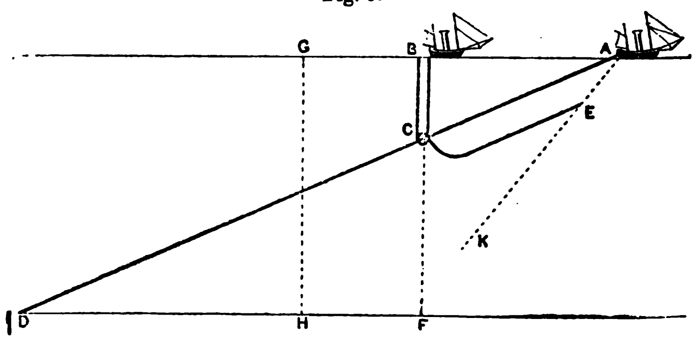
In spite of every precaution, and the most complete and well-devised system of paying-out apparatus, some accident may occur, even when the task is all but accomplished, resulting in delay, and

the loss, probably, of the cable itself. A fracture may take place from some unlooked-for accident even when within a few miles of the destination, and 2,000 miles of cable lie uselessly, and perhaps irrecoverably, at the bottom of the sea.

A simple and effective plan for recovering the end of the cable is therefore a very important subject for consideration. The arrangement about to be described appears to the Authors likely to succeed in securing the end of the cable in case of fracture. It is proposed that a second vessel shall follow in the wake of the paying-out vessel, at such a distance that it may always be about 200 fathoms above the cable. From this vessel is to be suspended a link, through which the cable should pass freely. This link should be suspended in such a manner that at any moment, upon a signal being given from the paying-out vessel, it might be reversed, or caused to rotate in a vertical plane, so as to make a single, double, or threefold hitch in the cable. By this means the further passage of the cable through the link would be arrested. The link and cable are then to be lowered to the bottom, and the following vessel is to put about, and be moved gently in the opposite direction, at such a rate as to insure the link and the table attached to it rising vertically to the surface, when the link is hauled up.

This is best illustrated by fig. 9, in which A and B are the

Fig. 9.

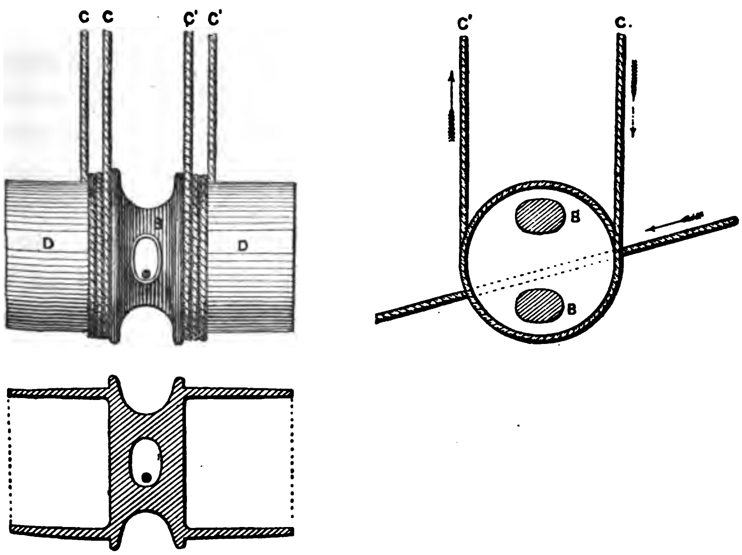


ordinary positions of the vessels, and A C D the line of cable, C being the link. When the link is reversed the motion of the cable through C is momentarily arrested, and the portion of the cable

A C comes into some position such as C E; the link C is then lowered to F. Then the vessel B is turned round, and goes slowly back to G, during which time the link is gradually raised, and finally the cable is brought into the position G H, where it may be spliced, and the paying-out proceed as before. The form of the link and the method by which it is proposed to make it rotate are shown in fig. 10, in which D D are two drums of cast-iron, connected by an intermediate piece B B, forming the link through which the cable passes. The whole is suspended by wire ropes C C′, which are coiled round each drum in such a manner that when C′ is hauled up C descends, and thus the drums and link are caused to rotate, and the cable is jammed between the cheeks B B by as many turns of the apparatus as may be deemed necessary.

The strain on the cable being greatest at A (fig. 9), it is

Fig. 10.



probable that the fracture would take place near that point, and by the formula given in the Appendix it is easy to calculate the direction of motion of the point A, which would not be vertical, but in an oblique direction, shown by the dotted line A K.

For greater security two or more following vessels might be employed, each carrying its check-link suspended at the depth due to its position.

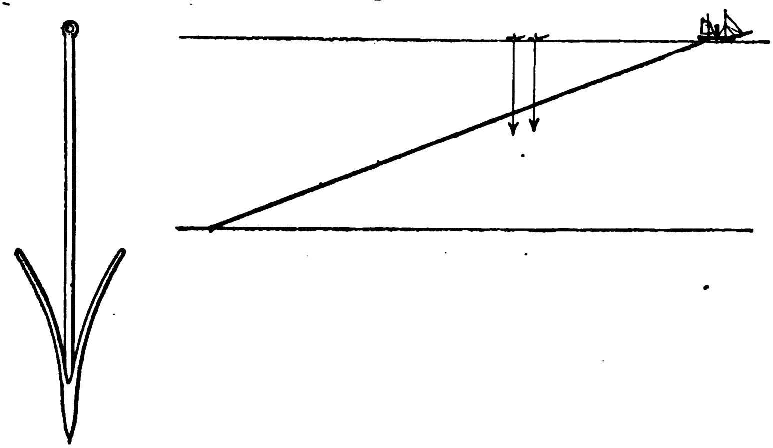
The Authors, whilst suggesting the above plan, freely admit that it is open to objection. Indeed they feel strongly that any apparatus connected with the cable after it leaves the paying-out vessel is undesirable, as introducing another possible cause of accident from some unforeseen derangement or unlooked-for neglect.

The objections which occur to them, to the apparatus above described, will now be pointed out, and it must be left to others to decide whether they are of sufficient importance to lead to the rejection of the plan. First, there is a possibility of the cable fouling, either with the suspended drums or with the suspending-ropes. This may be guarded against by care in paying-out, so as not to allow the cable too much slack. The risk of the catching apparatus twisting may, the Authors think, be avoided by the suspending-ropes being brought up to the opposite sides of the following-vessel, so that any tendency to twist would be resisted, on the principle of the bifilar mode of suspension in the torsion balance. Secondly, a danger might occur if a kink went overboard. This, perhaps, might be met by increasing the opening through the link between the drums so as to allow a kink to pass; but, if not, it does not follow that a fracture would take place. The suspending-ropes of the link should be so arranged as not to admit of any considerable increase of tension, and in case of fouling, or of kinks which could not pass through the apparatus, the paying-out should be stopped and the link lowered to the bottom, the cable being afterwards drawn up, as before detailed, in case of fracture. Doubtless this might involve some loss of time, and it necessitates an additional amount of personal attention; but the question is whether a better mode can be devised, and, if not, is the possible loss of time and the extra attention compensated for by the probable safety of the cable in case of fracture?

A different method of catching the cable might, perhaps, be adopted with success, if the cable were not too heavy. At a certain distance behind the paying-out vessel, two small steamers might

follow, keeping as nearly as possible in the line of the sinking cable. From each of them might be suspended one or more grapnels, with four or six arms, rising upwards at an acute angle (fig. 11). These grapnels should be suspended in the water at a depth a little

Fig. 11.



below the sinking cable. In case of fracture, upon a signal from the paying-out vessel, the two small vessels should at once steam at right angles to their former course, in opposite directions, so that one of them should cross the line of cable, which would probably be caught and jammed in the acute angle of the grapnel. As soon as this was done, which would be known by the increase of tension on the suspending-rope of the grapnel, this rope should be slacked out so as to avoid fracture from any undue catenarian strain. The ship's course should then be so altered as to cause the suspending-rope to hang vertically, with only such tension as is due to its own weight, to that of the grapnel, and to that of the cable between it and the bottom. The hauling-in would then be proceeded with, care being taken always to direct the ship's course so that the suspending-rope might hang vertically. With a light cable this method might succeed. It would not be so certain in its action as the first proposed method, but, on the other hand, it is less liable to objection, as a cause which might lead to fracture, from fouling, kinks, &c. The depths at which the grapnels should hang, and the distances of the following-vessels, would be regulated by the

specific gravity of the cable and the speed of the paying-out vessel. Tables are given in the Appendix to show the position of the cable relatively to the following-vessels, at the end of certain intervals of time after fracture. These Tables further show the time after fracture when the end of the cable would pass beneath the following-vessel; first, on the supposition of that vessel continuing her course, and secondly, on the supposition of her immediately crossing the line of cable when fracture was signalled. These Tables are calculated from the formula in the Appendix, Problem IX., for two different cables, and could of course be extended to any other conditions of a particular case. Instead of suspending the grapnels below the cable it might perhaps be preferable to suspend them a little above it, and lower them upon the signal of fracture being given.

XII. What is the best Mechanical Construction of a Submarine Telegraphic Cable?

In venturing a few remarks upon the construction of the cable itself the Authors beg to say that they only claim for their opinions the merit of being legitimate deductions from the foregoing investigations, entered upon without prejudice, and followed out to the best of their ability. If the investigations can be shown to be defective in principle, or to be inapplicable, the opinions based on them will be of comparatively little importance; but on the other hand, if the reasoning, which is given in the Appendix, be correct, the Authors claim for these deductions a positive practical value.

The information which has been published respecting the recent great experiment is but scanty, and there is much that is contradictory in the various accounts. The Authors have, therefore, refrained from entering into any review of the published statements, or from attempting to give a reason for the failure, beyond a passing suggestion, that the stoppage of the paying-out apparatus might be the cause. They would, before going further, only suggest that a minute detail of all the proceedings, and of the phenomena observed, both electrical and mechanical, would

form a very interesting and valuable communication to the Institution.

In the construction of a submarine cable there are three principal matters to be attended to:—

1st. Its conducting power.
2nd. Its insulation.
3rd. Its mechanical structure and condition.

The two former do not enter within the province of this Paper, but the latter does so eminently.

There are two descriptions of cable which have come under the notice of the Authors. In the first, the conducting medium is placed in the centre; next to it comes the insulating medium, and outside of all that which is to give it at once protection and strength. In the other, the strength and the conducting medium are one and the same, and are placed in the centre, being surrounded by the insulating medium. The former may be called the heavy, the latter the light, system of cable. The investigations of the Authors lead them to give a decided preference to the light system. Taking the Atlantic cable as a type of the heavy system, and as a very perfect one, inasmuch as those who have had to lay it declare that they can offer no suggestion for its improvement, it is found that with a weight in water of 15½ cwts. to the mile it offers a tensile strength of about 4¼ tons; that is to say, it will support a length of 5½ miles of itself, hanging vertically in water. Taking, again, the cable proposed by Mr. Allan as a type of the light cable, it is found that with a weight in water of 3¼ cwts. to the mile it offers a tensile strength of 2 tons, or will support a length of 12½ miles of itself hanging vertically in water. Consequently the light cable has to the heavy cable a relative strength of 25 to 11.

In the next place, the weights in air are about 21½ cwts. and 10 cwts. to the mile respectively, being an economy, as regards transport, of 54 per cent. in favour of the light cable.

Another question for consideration is, the effect of tension and compression on the two descriptions of cable. The heavy cable is composed of two independent metallic portions, separated by the

insulating medium. The outer casing is wound spirally round, and it is clear that the effect of tension must be to stretch it, and so to strain both the insulating medium and the inner core. This extension is greater than that ordinarily due to tension, from the following cause. In great depths of water the pressure upon the cable is very considerable; for instance, in 2,000 fathoms it is about $2\frac{1}{2}$ tons per square inch. Now, under such a pressure, it is to be expected that the insulating medium will be compressed. This being so, the cable will be reduced in diameter, and the spiral strands on the outside will adjust themselves to the new diameter, and the angle of the spirals becoming more acute, the outer shell will increase in length. The proportion of this increase may be calculated, as shown in the Appendix, Problem X., whence it appears it is $\frac{\pi^2 d\, \delta}{l^2}$ nearly, where d is the original diameter from centre to centre of the spiral strands, δ the decrease of diameter due to compression, and l the length in which the strands make an entire turn round the cable. In the Atlantic cable d is about 0·5 of an inch, l is 9 inches, and if $\delta = 0{\cdot}1$ of an inch, the increase of length would be about one-eighteenth part of its length, and this, it must be borne in mind, is altogether independent of the stretching due to tension.

The Authors are not aware to what extent gutta-percha is compressible, nor have they any information respecting the amount of stretching of the Atlantic cable under a given tension. They therefore content themselves with pointing out the two causes of tension and compression, as both resulting in a stretching of the outer shell, whilst the insulation and the inner core are not thus acted on. It is possible that the amount of such stretching, and its consequent strain on the inner part of the cable, may not be of serious moment within the limits of the tension due to the laying of the cable, but it must not be forgotten that undue strains may easily be brought on by any fouling or imperfect action of the paying-out apparatus, and it has been shown that even at a rate of 6 feet per second such strain may easily amount to 7 tons. It is, therefore, perhaps not going too far to say, that this structure of cable may, possibly, have its conducting power, or its insulation,

seriously injured by the stretching of the outer shell, although no absolute fracture may take place. From such a contingency the other description of cable is free, and it would probably remain uninjured by any amount of compression, or by any tension, within the limits of its tensile force.

As regards the protection given by the outer metallic casing, it has been stated that it is only designed to protect the inner core from mechanical violence, and to confer on the cable a convenient amount of proportionate weight during the process of submergence, and that when once laid at the bottom the rust may eat up the external coat. The Authors submit, that there can be no practical difficulty in protecting the lighter cable from mechanical violence, by giving it an outer coating of hempen cord, as has been proposed, and their investigations have led them to the conclusion that the increase of weight due to the outer casing of the other cable is not an advantage or convenience, but quite the contrary, inasmuch as it necessitates the application of a greatly-increased resistance of the breaks whilst being laid down. But, even if weight is desirable, they would observe, that what they had termed the light system admits of its being made of any desired specific gravity; and if made as heavy as the Atlantic cable it would still possess the advantage of having the whole of the metallic material in the centre, instead of partly in the centre and partly in the circumference.

The relative cost of the two kinds of cable is also a matter for consideration. The Authors are informed that on a light cable of the same power as the Atlantic cable there would be a saving of about thirty per cent. in first cost.

In every point of view which has fallen under their notice the Authors feel bound to give their decided opinion in favour of the light system of cable. Whether there are any objections to it electrically they are not prepared to say; but finding that so long ago as 1853 a cable of this description was proposed and advocated for the express purpose of crossing the Atlantic, curiously enough under the very title of "The Atlantic Telegraph," the Authors cannot but think that there must exist some reason unknown to them why a cable more expensive, and more difficult to manage,

was adopted. They venture to hope that this Paper may be the means of eliciting some information on the subject, which cannot but be interesting to the profession at large.

In now bringing this communication to a close the Authors desire to state, that, though they have referred to the Atlantic cable and to Mr. Allan's cable as types of two distinct systems of construction, they must disclaim any intention of imputing carelessness to those who undertook the late experiment, which so unfortunately failed. The magnitude of the operation removes it, to a great extent, beyond the pale of previous experience, and as such, those engaged in it are entitled to the sympathy of all generous minds. This they doubtless have received, and the Authors trust that there will not be ascribed to them any disposition to cavil or blame, when as the result of their investigation they have felt themselves compelled to express the opinion freely, that though the Atlantic cable is a step in the right direction, as compared with the heavier cables of former days, it yet falls far short in mechanical structure and in condition of the light system.

Still less would the Authors be thought to deny the practicability of submerging the present Atlantic cable; on the contrary, they have no hesitation in saying, that with proper precautions, and a due attention to what is required in the construction of the paying-out apparatus, the submerging may probably be successfully accomplished; but they cannot too earnestly repeat their conviction, that with the present cable the success of the operation will mainly depend upon the nature of the paying-out machinery and the general mechanical arrangements.

The Authors hope that the free expression of their own opinions, and of the grounds on which they are based, will lead to a like free expression of the opinions and experience of others in the discussion that will probably ensue; and that, however imperfect may be the present treatment of the subject, it may contribute to diffuse a better knowledge of the principles, upon the following out of which the successful result of such undertakings mainly depends.

The Paper is illustrated by a series of diagrams, from which ;s. 1 to 16 are compiled; and is accompanied by the mathe-atical investigations at length, of which an abstract is given in .e Appendix.

APPENDIX.

PROBLEM I.

Equations of motion of a body descending in a resisting medium.

Let a = the area of the horizontal section of the unit of length.
A = the volume of the body for the unit of length.
c = the coefficient of resistance, depending on the form of the body.
s = the specific gravity of the medium.
s' = the specific gravity of the sinking body.
ν = the initial velocity of the descent.
v = the velocity of the descent at any time.
t = the corresponding time.
x = the corresponding space.
g = the accelerating force of gravity = 32·2.
ϵ = the base of the Napierian logarithms.

Then, the accelerating force of the body $= g\,\dfrac{s'-s}{s'}$,

the resistance at v $\quad \dfrac{c\,a\,s\,v^2}{2g}$,

the retarding force $= \dfrac{c\,a\,s\,v^2}{2g} \times \dfrac{g}{A\,s'}$

and, in the case of a cable laid horizontally, and when the diameter $= d$,

$$A = \frac{d^2\pi}{4},\ a = d,\ c = \frac{2}{3};$$

therefore the retarding force $= \dfrac{c\,a\,s\,v^2}{\dfrac{2\,d^2\pi\,s'}{4}} = \dfrac{4\,s\,v^2}{3\pi\,d\,s'}$

and, therefore, the actual force of the descent $= g\,\dfrac{s'-s}{s'} - \dfrac{4\,s\,v^2}{3\pi\,d\,s'}$.

Also, for a sphere whose diameter $= d$,

$$A = \frac{d^3\pi}{6},\ a = \frac{d^2\pi}{4},\ c = \frac{1}{2};$$

whence, the actual force of the descent $= g\,\dfrac{s'-s}{s'} - \dfrac{3\,s\,v^2}{8\,d\,s'}$.

Now, $v\,dv = f\,ds$, therefore, for the cable, $v\,dv = \left(g\,\dfrac{s'-s}{s'} - \dfrac{4\,s\,v^2}{3\pi\,d\,s'}\right)dx$;

and making $c = \dfrac{4\,s}{3\pi\,d\,s'}$, and $n = \sqrt{\dfrac{3\,g\,\pi\,d}{4} \cdot \dfrac{s'-s}{s}}$,

and integrating and correcting, by making $v = \nu$, when $x = o$,

$$v = \sqrt{n^2 - (n^2 - \nu^2)\,\epsilon^{-2cx}} \quad \ldots\ldots\ldots\ldots \quad (1).$$

Again, $dt = \frac{dx}{v} = \frac{1}{c} \frac{dv}{n^2 - v^2}$,

not integrating and correcting, by making $v = \nu$, when $t = o$,

$$t = \frac{1}{2cn} \log_\varepsilon \left\{ \frac{n+v}{n-v} \cdot \frac{n-\nu}{n+\nu} \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

and, substituting (1) in (2),

$$t = \frac{1}{2cn} \log_\varepsilon \left\{ \frac{n + \sqrt{n^2 - (n^2 - \nu^2)\,\varepsilon^{-2cx}}}{n - \sqrt{n^2 - (n^2 - \nu^2)\,\varepsilon^{-2cx}}} \cdot \frac{n-\nu}{n+\nu} \right\} \quad . \quad . \quad . \quad (3).$$

The equations for a sphere are similar in form, but the constants are

$$c = \frac{3s}{8ds'}$$

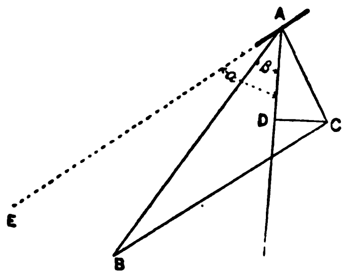
$$n = \sqrt{\frac{8gd}{3} \cdot \frac{s'-s}{s}}.$$

Cor. From (1), it appears that when x is large, $v = n$ very nearly.

Problem II.

Equations of motion of a body descending uniformly in an oblique position through a resisting medium.

Fig. 12.



Let A be a particle of the body.

α the angle made by A and the vertical.

β the angle made between the path of the descent A B and the vertical A D

$v =$ the velocity in A B.

$q =$ the coefficient for the resistance of the water to the unit of length, at t velocity of 1 foot per second.

$q' =$ the coefficient for friction in the unit of length at 1 foot per second.

$w =$ the weight of A in water.

Then, the resistance of the water in B A may be resolved into A C at right angl to A, which may be again resolved into two forces, viz.,

Force in A D $= q\, v^2 \sin^2 (\alpha - \beta) \sin \alpha$ up;
Force in C D $= q\, v^2 \sin^2 (\alpha - \beta) \cos \alpha$ to left.

Also friction in A $= q'\, v^2 \cos^2 (\alpha - \beta)$;
which may be resolved into

$q'\, v^2 \cos^2 (\alpha - \beta) \cos \alpha$ vertically up;
$q'\, v^2 \cos^2 (\alpha - \beta) \sin \alpha$ to the right.

Therefore, since the motion is uniform,

$$w - q\,v^2 \sin^2(\alpha - \beta)\sin\alpha - q'\,v^2\cos^2(\alpha - \beta)\cos\alpha = o,$$
$$q\,v^2\sin^2(\alpha - \beta)\cos\alpha - q'\,v^2\cos^2(\alpha - \beta)\sin\alpha \qquad = o;$$

from which equations,

$$v = \sqrt{w\left(\frac{\cos\alpha}{q'} + \frac{\sin\alpha}{q}\right)} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1).$$

$$\beta = \alpha - \tan^{-1}\sqrt{\frac{q'}{q}\tan\alpha} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2).$$

Cor. 1. The velocity of running out vertically, without tension, is found by making $\alpha = o$, whence $v = \sqrt{\frac{w}{q'}}$.

Cor. 2. The angle at which the cable would run out with the greatest velocity may be found by making $\sqrt{w\left(\frac{\cos\alpha}{q'} + \frac{\sin\alpha}{q}\right)}$ a maximum from which $\tan\alpha = \frac{q'}{q}$ and $\beta = o$.

Cor. 3. By this problem may also be found the waste of cable when it runs out at any given angle (α) free from tension:

$$\text{Waste per cent.} = 100\,\frac{\sec\alpha - \tan\alpha + \tan\beta}{\sec\alpha}.$$

Cor. 4. Also, the angle of motion (β) of the end, in case of fracture, may be found, for

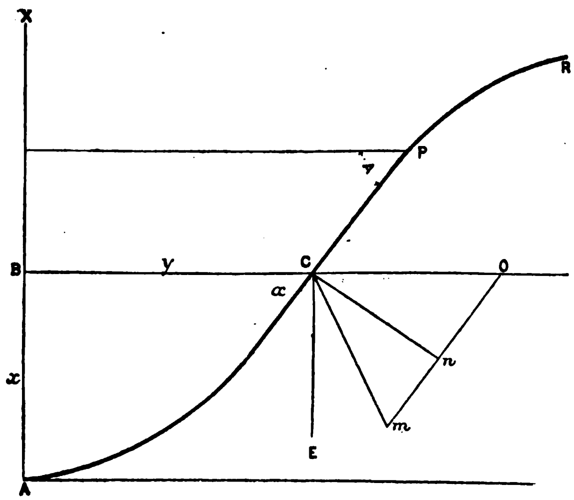
$$\tan(\alpha - \beta) = \sqrt{\frac{q'}{q}\tan\alpha},$$

when α is given.

Problem III.

General equation to the curve assumed by the sinking cable.

Fig. 13.



Let A C P R be a portion of the curve.

x = any abscissa.
y = the corresponding ordinate.
s = the corresponding length of the curve.
α = the angle with the horizon at C.
A = the angle at any other point, P.
a = the length of the cable whose weight in water is equal to the horizontal tension at A.
t = the tension at P, in the direction of the curve towards R.
v = the velocity of the paying-out vessel.
m = the coefficient of the resistance of the water at the velocity v.
m' = the coefficient of friction at the velocity v.
w = the weight in water of the unit of length.
d = the diameter of the cable in the same unit.

Then, it may be shown, in order that the length AC should be laid without slack along the bottom, that the point C must move in a direction C m bisecting the angle formed at C with the horizon; and this is true of every point in the curve.

Therefore the resistance of the water may be replaced at every point by a force of stream acting in the direction bisecting the angle with the horizon, and with a velocity equal to the velocity of the point in that direction.

Taking any point C,

$$\text{the velocity in C } m = 2\, v \sin\frac{\alpha}{2},$$

$$\text{therefore, the resistance in C } m = 4\, m \sin^2\frac{\alpha}{2};$$

but the cable is not at right angles to C m, as it forms an angle $= \dfrac{180 - \alpha}{2}$ with it; so that the resistance must be resolved into C n and n m; the former at right angles to the curve, and the latter parallel to it, and only acting by friction.

Whence

$$\text{the resistance in } n \text{ C} = 4\, m \sin\frac{2\,\alpha}{2} \sin^2\frac{180 - \alpha}{2}\, d\,s = m \sin^2 \alpha\, d\,s;$$

which must be again resolved into

$m \sin^2 \alpha \cos \alpha\, d\,s$ vertically up;
$m \sin^3 \alpha\, d\,s$ horizontally to the left.

Again, the velocity in the direction $m\,n = 2\, v \sin^2\frac{\alpha}{2}$, therefore the friction for the unit of length at c $m'\, 4 \sin^4\frac{\alpha}{2}$;

which must be resolved into (for $d\,s$)

$4\, m' \sin^4\frac{\alpha}{2} \sin \alpha\, d\,s$ vertically up;

$4\, m' \sin^4\frac{\alpha}{2} \cos \alpha\, d\,s$ horizontally to the right.

Also, the tension at P, or t, must be resolved into

$t \sin$ A vertically up;
$t \cos$ A horizontally to the right.

The remaining forces are

$w\,s$ vertically down ;
$w\,a$ horizontally to the left.

Therefore, collecting the forces,

$$t \sin A + m \int_o^s \sin^2 \alpha \cos \alpha \, d s + m' \int_o^s 4 \sin^4 \frac{\alpha}{2} \sin \alpha \, d s - w s = o \quad . \quad . \quad (1),$$

and

$$t \cos A - m \int_o^s \sin^3 \alpha \, d s + m' \int_o^s \sin^4 \frac{\alpha}{2} \cos \alpha \, d s - w a = o \quad . \quad . \quad . \quad . \quad . \quad (2);$$

from which, by taking s up to P,

$$\frac{d t}{d s} w \sin A + m' \, 4 \sin^4 \frac{A}{2} = o \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

$$t \frac{d A}{d s} - w \cos A + m \sin^2 A = o \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Now, $\sin A = \frac{d x}{d s}$, and by substituting in (3), integrating and correcting,

$$t = w\,(x + a) - m' \frac{(1 - \cos A)^2}{\sin A} x \text{ nearly} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5),$$

which is the equation for tension.

Again, from (4),

$$t \frac{d A}{d s} - w \cos A + m \sin^2 A = o; \text{ but } A = \cot^{-1} \frac{d y}{d x};$$

therefore, $t \frac{d A}{d s} = - t \frac{d^2 y}{d x^2} \cdot \left(\frac{d x}{d s}\right)^3$,

and $- t \frac{d^2 y}{d x^2} - w \frac{d y}{d x} \left(\frac{d s}{d x}\right)^2 + m \frac{d s}{d x} = o$;

and writing $p = \frac{d y}{d x} = \cot A$,

$$- t \frac{d p}{d x} - w p \,(1 + p^2) + m \sqrt{1 + p^2} \ldots o;$$

and by writing $\frac{1}{q} = p$, and $\sqrt{1 + q^2} = z$, successively,

$$- \frac{d x}{t} = \frac{d z}{m z^2 - w z - m} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

But (5), $t = w\,(x + a) - m' \frac{1 - \cos A)^2}{\sin A} x$;

therefore

$$- \frac{d x}{t} = \frac{- d x}{w\,(x + a) - m' \frac{(1 - \cos A)^2}{\sin A} x} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

Equating (6) and (7),

$$- \frac{d x}{w\,(x + a) - m' \frac{(1 - \cos A)^2}{\sin A} x} = \frac{d z}{m z^2 -}$$

and integrating and correcting, and writing fo

finally, that

$$\frac{1}{w - m' \frac{(p - \sqrt{1 + p^2})^2}{\sqrt{1 + p^2}}} \log \frac{w a}{w a + \left\{w - m' \frac{(p - \sqrt{1 + p^2})^2}{\sqrt{1 + p^2}}\right\} x} = \frac{1}{\sqrt{w^2 + 4 m^2}}$$

$$\log \left\{ \frac{2m \sqrt{1 + \frac{1}{p^2}} - w - \sqrt{w^2 + 4m^2}}{2m \sqrt{1 + \frac{1}{p^2}} - w + \sqrt{w^2 + 4m^2}} \cdot \frac{2m - w + \sqrt{w^2 + 4m^2}}{2m - w - \sqrt{w^2 + 4m^2}} \right\} \quad . \quad . \quad (8).$$

An equation between x and $\frac{dy}{dx} = \cot A$, from which the form of the curve may be derived.

If m' be neglected, or made $= o$, y may be obtained in terms of a series containing x, or

$$y = \frac{x}{\sqrt{\tau}} - \frac{a \cdot \frac{\tau + 2}{\tau \sqrt{\tau}} \cdot \frac{S - 1}{D + 1} \cdot \left(\frac{a}{a + x}\right)^{\sqrt{1 - \frac{4m^2}{w^2}} - 1}}{\sqrt{1 + \frac{4m^2}{w^2}} - 1}$$

$$- \frac{a \frac{4\tau + 12 - \tau\theta}{2\tau^2 \sqrt{\tau}} \cdot \left(\frac{S - 1}{D + 1}\right)^2 \left(\frac{a}{a + x}\right)^{2\sqrt{1 + \frac{4m^2}{w^2}} - 1}}{2\sqrt{1 + \frac{4m^2}{w^2}} - 1} - \&c.\ \&c. + C_1 \quad . \quad (9).$$

When $S = \frac{w + \sqrt{w^2 + 4m^2}}{2m}$, $D = \frac{-w + \sqrt{w^2 + 4m^2}}{2m}$,

$\tau = S^2 - 1$,

and $\theta = D^2 - 1$.

When $x =$ infinity, this gives $y = \frac{x}{\sqrt{\tau}} + C_1$, whence $\frac{dx}{dy} = \sqrt{\tau}$, or the inclination of the asymptote to the horizon $= \tan^{-1} \sqrt{\tau} = \cos^{-1} \frac{\sqrt{w^2 + 4m^2} - w}{2m}$ . . (10).

When $a = o$,

$y = \frac{x}{\sqrt{\tau}} + C_1$, which is the equation to a straight line inclined to the horizon at an angle $\cos^{-1} \frac{\sqrt{w^2 + 4m^2} - w}{2m}$ . . . . (11).

Which value might also have been derived from equation (8).

From this it appears, that, when a cable is laid without tension at the bottom, it will descend in a straight line, inclined at the above angle to the horizon.

PROBLEM IV.

Equation for tension.

From (5), Problem III., $t = w(x + a) - m' \frac{(-\cos A)^2}{\sin A} x$,

which, when $a = o$, becomes

$$t = w x - m' \frac{(1 - \cos A)^2}{A} x.$$

If v is the velocity of the cable running out, and ν the velocity of the paying-out vessel, then

$$t = wx - m' \frac{\left(\frac{v}{\nu} - \cos A\right)^2 x}{\sin A} \quad . \quad . \quad . \quad (1);$$

but m' is a function of ν, say $= q' \nu^2$,
whence

$$t = wx - q' \nu^2 \left(\frac{v}{\nu} - \cos A\right)^2 \operatorname{cosec} A \,.\, x \quad . \quad . \quad . \quad (2).$$

The ratio of v to ν, which is required in order to obtain any given amount of tension t', will be

$$\frac{v}{\nu} = \cos A + \sqrt{\frac{wx - t'}{q' \nu^2 x}} \sin A \quad . \quad . \quad . \quad (3).$$

The following Tables have been calculated for two cables, the one being the Atlantic cable, and the other a cable of the same diameter, but with a specific gravity of 1·5.

Atlantic Cable.

Specific gravity = 3·489, Diameter = $\frac{5}{8}$ths of an inch, Weight of 1 foot in water = 0·3208 lbs., Depth of Water = 2000 fathoms, and Tension, when vertical, = 3849·6 lbs.

		Tensions in lbs.						
Velocity of the Vessel in Feet per Second		2	4	6	8	10	12	15
Angle of Inclination to the Horizon		68° 37′	41° 44′	28° 45′	21 47′	17° 31′	14° 38′	11° 44′
Ratio of paying out to Velocity of Vessel	1	3839	3839	3842	3844	3845	3846	3846
Ditto	$\frac{5}{4}$	3828	3806	3781	3733	3658	3550	3316
Ditto	$\frac{6}{4}$	3813	3760	3658	3481	3198	2763	1876
Ditto	$\frac{7}{4}$	3795	3690	3474	3085	2464	1556	−483
Ditto	2	3774	3599	3227	2548	1457	−143	...

Light Cable.

Specific gravity = 1·500, Diameter = $\frac{5}{8}$ths of an inch, Weight of 1 foot in water = 0·06578 lbs., Depth of water = 2000 fathoms, and Tension, when vertical, = 789·9 lbs.

		Tensions in lbs.					
Velocity of the Vessel in Feet per Second		4	6	8	10	12	15
Angle of Inclination to the Horizon		19° 56′	13° 21′	[illegible]	[illegible]	[illegible]	[illegible] 22′
Ratio of paying out to velocity of Vessel	1	798	798·2	[illegible]	[illegible]	[illegible]	[illegible]
Ditto	[illegible]	[illegible]	775	[illegible]	[illegible]	[illegible]	[illegible]
Ditto	[illegible]	[illegible]	720	[illegible]	[illegible]	[illegible]	[illegible]
Ditto	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Problem V.

To find the waste of cable from currents.

It is shown that if

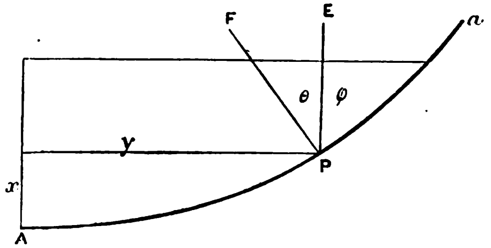
$d =$ the depth to which the current extends,
$v =$ the velocity of the current,
$\nu =$ the velocity of the ship,
$A =$ the angle of inclination of the cable to the horizon, the

$$\text{waste} = \frac{d}{\nu \sin A}\left\{\nu - \sqrt{\nu^2 - v^2} + \cos A\left(\nu - \sqrt{\nu^2 - \frac{v^2}{\cos^2 A}}\right)\right\}$$

Problem VI.

Equation to the curve assumed by a flexible line stretched across a current.

Fig 14.



Let $\phi =$ the angle between the curve and the direction of the current at any point P.

$\theta =$ the angle between the direction of the current and the normal to the curve at P.

$t =$ the tension at P.

$a =$ the tension at A.

$f =$ the force of the current on an unit of surface at right angles to its direction.

Then, at any point P, f may be resolved into

$f \sin^2\varphi \cos\theta$ in the direction of the current.
$f \sin^2\varphi \sin\theta$ perpendicular to the current.

Also t may be resolved into

$t \cos\varphi$ in the direction of the current.
$t \sin\varphi$ perpendicular to the current.

Therefore,

$$\int_0^s f \sin^2\varphi \cos\theta \, ds - t \cos\varphi = o \quad \ldots \ldots \quad (1).$$

$$\int_0^s f \sin^2\varphi \sin\theta \, ds + t \sin\varphi - a = o \quad \ldots \ldots \quad (2).$$

From which, as $\phi + \theta = \frac{\pi}{2}$,

$$\frac{dt}{ds} = o \quad \text{. . . . . (3).}$$

$$f \sin^2\phi \, d s = - t \, d \phi \quad \text{. . . . . (4).}$$

From (3) it appears that the tension is constant, and, since it is $= a$ at the vertex, that must be its value at any other point;

therefore from (4) $f \sin^2\phi \, d s = a \, d x$, but $\sin \phi = \frac{d y}{d s}$,

and substituting and integrating

$$f s = a \frac{d x}{d y} + c;$$

but when $s = o$, $\frac{d x}{d y} = o$, therefore $c = o$,

and $\frac{a}{f s} = \frac{d y}{d x}$, which is the equation to the common catenary.

It therefore appears, that the form of the curve is the common catenary, and that the tension is uniform throughout.

Problem VII.

To find the tension due to the friction of the water in a current.

In Problem VI. the effect of friction was neglected. It may, however, easily be obtained.

If d is the depth of the current,
 v the velocity of the current,
 $\nu =$ the velocity of the ship,
 $q' =$ the coefficient of friction for one lineal foot of cable, at 1 foot per second,
 $A =$ the angle of the cable with the horizon;

then, the total friction on the cable $= q' \, d \frac{v^4}{\nu^2} \operatorname{cosec} A$.

In the case of the Atlantic cable, if

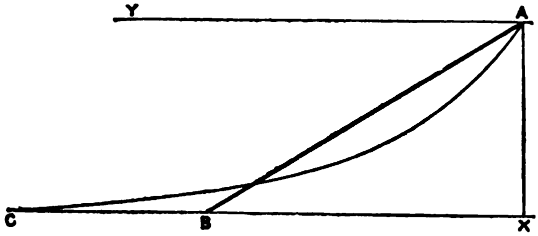
$\nu = 6$ feet per second,
$v = 1\frac{1}{2}$ foot per second,
$d = 12{,}000$ feet,
$q' = \cdot 00054778$ lbs.,
$A = 28° \, 45'$;

then, the total friction $= 0{\cdot}1922$ lbs., showing that the effect is quite insignificant.

Problem VIII.

To find the form the cable will assume, and the tension, in case the paying-out vessel and the paying out should be suddenly stopped.

Fig. 15.



Let A B be the line of the cable whilst the paying out proceeds.

And let $AB = s$.

$AX = x$, the depth of the water.

$XC =$ the distance horizontally from X to C, the origin of the curve assumed by the cable after the stoppage.

$l =$ the length of the curve A C when equilibrium is established.

$a =$ the tension at C.

$t =$ the tension at A.

$\alpha =$ the angle formed by the curve A C with the horizon at A.

Then, $l = AB + BC = s + y - \sqrt{s^2 - x^2}$ . . . . (1).

But by the equations to the catenary

$$l = t \sin\alpha \quad \ldots\ldots \quad (2).$$

$$x = t(1 - \cos\alpha) \quad \ldots\ldots \quad (3).$$

$$y = t\cos\alpha \log\frac{\cos\alpha}{1 - \sin\alpha} \quad \ldots\ldots \quad (4).$$

Equating (1) and (2)

$$t\sin\alpha = s + y - \sqrt{s^2 - x^2},$$

but (3) $t = \frac{x}{1 - \cos\alpha}$, therefore

$$\frac{x\sin\alpha}{1 - \cos\alpha} = s + y - \sqrt{s^2 - x^2}$$

$$= s - \sqrt{s^2 - x^2} + \frac{x}{1 - \cos\alpha}\cos\alpha\log\frac{\cos\alpha}{1 - \sin\alpha} \quad \ldots\ldots \quad (5).$$

But s and x are given; therefore, from this equation α may be found, and then

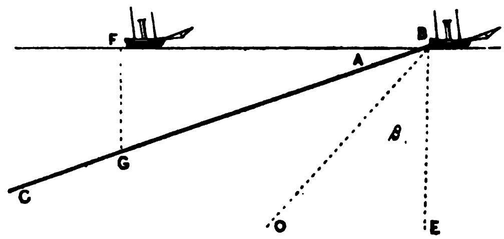
$$t = \frac{x}{1 - \cos\alpha}$$

$$y = \frac{x}{1 - \cos\alpha}\cos\alpha\log\frac{\cos\alpha}{1 - \sin\alpha}.$$

Problem IX.

To find the depth of the cable below a following vessel, at any interval after fracture.

Fig. 16.



Let B C be the line of the cable paying out.

B O, the direction of motion after fracture,

$d = \mathrm{BF} =$ the distance of the vessels apart.

Then $\mathrm{FG} = d \sin \mathrm{A} =$ the depth before fracture,

$v =$ the velocity of the end after fracture, as obtained from Problem II.; then $v \cos \beta =$ the vertical component of the velocity, and the depth at the end of t seconds will be

$$= d \sin \mathrm{A} + v \cos \beta \,.\, t;$$

but in t'' the vessel has moved forward $\nu\, t$, and decreased the distance between it and the cable by $\nu\, t \sin \mathrm{A}$; therefore the depth of the cable below F, at the end of t'',

$$= d \sin \mathrm{A} + t\,(v \cos \beta - \nu \sin \mathrm{A}).$$

The following Tables have been calculated, by means of this formula, for two cables, at various speeds of paying out.

Atlantic Cable; Velocity of the Vessel 6 Feet per Second.

Time after Fracture . .		0″	10″	20″	30″	40″	50″	60″	70″	80″	90″	100″	120″	A[1]	B[1]
Distance between the Vessels .	ft. 300	144	222	299	.	.	.	.	.	.	.	.	.	13″	23″
Ditto . .	600	288	366	443	520	.	.	.	.	.	.	.	.	27	46
Ditto . .	900	433	510	587	664	741	.	.	.	.	.	.	.	41	70
Ditto . .	1200	577	654	731	808	886	963	.	.	.	.	.	.	55	93

Atlantic Cable; Velocity of the Vessel 8 Feet per Second.

Time after Fracture . .		0″	10″	20″	30″	40″	50″	60″	70″	80″	90″	100″	120″	A[1]	B[1]
Distance between the Vessels .	ft. 300	111	164	217	.	.	.	.	.	.	.	.	.	13″	24″
Ditto . .	600	223	275	327	380	.	.	.	.	.	.	.	.	27	48
Ditto . .	900	334	387	439	492	544	.	.	.	.	.	.	.	40	72
Ditto . .	1200	445	498	550	603	656	709	.	.	.	.	.	.	54	95

[1] Column A gives the time, in seconds, after fracture, when the end of the cable passes beneath the following vessel, the following vessel continuing her course. Column B gives the time of passing beneath the following vessel, on the supposition of the vessel being stopped at the instant of fracture.

Light Cable; Velocity of the Vessel 6 Feet per Second.

Time after Fracture . .		0"	10"	20"	30"	40"	50"	60"	70"	80"	90"	100"	120"	A[1]	B[1]
Distance between the Vessels .	ft. 300	69	81	93	105	.	.	.	.	.	.	.	.	30"	62"
Ditto . .	600	139	151	163	175	187	199	211	.	.	.	.	.	61	124
Ditto . .	900	208	220	232	244	256	267	279	291	303	315	.	.	92	186
Ditto . .	1200	277	289	301	313	325	337	349	361	373	385	397	421	122	250

Light Cable; Velocity of the Vessel 8 Feet per Second.

Time after Fracture . .		0"	10"	20"	30"	40"	50"	60"	70"	80"	90"	100"	120"	A[1]	B[1]
Distance between the Vessels .	ft. 300	52	60	68	.	.	.	.	.	.	.	.	.	24"	70"
Ditto . .	600	104	112	120	128	136	144	.	.	.	.	.	.	48	140
Ditto . .	900	157	165	173	181	189	197	205	213	.	.	.	.	72	210
Ditto . .	1200	209	217	225	233	241	249	257	265	273	281	289	.	96	280

Light Cable; Velocity of the Vessel 10 Feet per Second.

Time after Fracture . .		0"	10"	20"	30"	40"	50"	60"	70"	80	90"	100"	120"	A[1]	B[1]
Distance between the Vessels .	ft. 300	42	47	53	.	.	.	.	.	.	.	.	.	21"	79"
Ditto . .	600	84	90	95	101	106	.	.	.	.	.	.	.	43	158
Ditto . .	900	126	132	137	143	148	153	159	.	.	.	.	.	64	237
Ditto . .	1200	168	173	179	185	190	196	201	207	213	218	.	.	85	316

Problem X.

To find the extension of length due to the compression of the inner core.

If $d =$ the original diameter from centre to centre of the outer strands,

$\delta =$ the decrease due to compression,

$l =$ the length of the cable in which the strands make an entire turn,

$L =$ the length of the strand making an entire turn,

then $l^2 = L^2 - \overline{\pi d}|^2$;

and if d becomes $d - \delta$, and l' the new value of l,

$$l'^2 = L^2 - \overline{\pi d}|^2 + 2\pi^2 d\delta - \pi^2\delta^2,$$

$$l' - l = \sqrt{l^2 + 2\pi^2 d\delta - \pi^2\delta^2} - l;$$

[1] Column A gives the time, in seconds, after fracture, when the end of the cable passes beneath the following vessel, the following vessel continuing her course. Column B gives the time of passing beneath the following vessel, on the supposition of the vessel being stopped at the instant of fracture.

and expanding the part under the radical sign,

$$l' - l = \frac{\pi^2 (2 d \delta - \delta^2)}{2 l} \text{ nearly,}$$

$$= \frac{\pi^2 \delta d}{l} \text{ nearly;}$$

$$\text{therefore } \frac{l' - l}{l} = \frac{\pi^2 d \delta}{l^2}.$$

Problem XI.

To find the variation of tension due to the motion caused by waves.

Let R = the normal tension due to the depth at the mean level, as given by Problem IV.,

T = the tension at any time t,

t = the time reckoned from the moment when the vessel is at the mean level,

t' = the total time of transit of a wave under the ship,

θ = the additional angular rotation caused by the rise and fall of the ship, and which may be either positive or negative,

x = the height of the ship above the mean level at t,

r = the radius of the sheaves,

ρ = the radius of gyration of the sheaves,

g = the accelerating force of gravity = 32·2,

h = the total height of the wave, and

w = the weight of the rotative machinery.

Since the moments of the impressed forces are equal to the moments of the effective forces,

$$(\mathrm{T} - \mathrm{R}) r = \Sigma m z^2 \frac{d^2 \theta}{d t^2}, \text{ but } \Sigma m z^2 = \frac{w}{g} \cdot \rho^2;$$

$$\text{therefore } (\mathrm{T} - \mathrm{R}) r = \frac{w}{g} \rho^2 \frac{d^2 \theta}{d t^2} \text{ and } \frac{d^2 \theta}{d t^2} = \frac{(\mathrm{T} - \mathrm{R}) r \cdot g}{w \rho^2} \quad \ldots\ldots \quad (1).$$

Now, if A is the angle of paying out at the mean level, it is shown that approximately the extra length paid out whilst the vessel rises to x is $x \sin \mathrm{A}$; therefore

$$x \sin \mathrm{A} = r \theta,$$

$$\text{and } r \frac{d^2 \theta}{d t^2} = \sin \mathrm{A} \frac{d^2 x}{d t^2} \text{ and } \frac{d^2 \theta}{d t^2} = \frac{\sin \mathrm{A}}{r} \cdot \frac{d^2 x}{d t^2}.$$

Hence from (1),

$$\mathrm{T} - \mathrm{R} = \frac{w \sin \mathrm{A} \rho^2}{g r^2} \cdot \frac{d^2 x}{d t^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2).$$

It is now necessary to find, or assume, x as some function of t, and it will probably be tolerably near the truth if it is assumed that

$$x = \frac{h}{2} \sin \left(2 \pi \frac{t}{t'}\right),$$

$$\text{whence } \frac{d x}{d t} = \frac{\pi h}{t'} \cos \left(2 \pi \frac{t}{t'}\right),$$

$$\frac{d^2 x}{d t^2} = - \frac{2 \pi^2 h}{t'^2} \sin \left(2 \pi \frac{t}{t'}\right),$$

therefore, from (2),

$$\mathrm{T} - \mathrm{R} = - \frac{2 \mathrm{w} \sin \mathrm{A} \rho^2 \pi^2 h}{g r^2 t'^2} \sin \left(2 \pi \frac{t}{t'}\right);$$

or, writing $m = \dfrac{2\,w \sin A_{\varrho}\, 2\pi^2 h}{g\, r^2\, t'^2}$.

$$T = R - m \sin\left(2\pi \frac{t}{t'}\right) \quad \ldots \ldots \quad (3).$$

Hence the variation of tension is $= 2\,m$, because $\sin\left(2\pi\frac{t}{t'}\right)$ may be either $+$ or -1.

From this the following corresponding values may be derived :—

t	x	Velocity of the Vessel Vertically.	Accelerating Force.
0	0	$\frac{\pi h}{t'}$	0
$\frac{t'}{4}$	$\frac{h}{2}$	0	$-\frac{2\pi^2 h}{t'^2}$
$\frac{t'}{2}$	0	$-\frac{\pi h}{t'}$	0
$\frac{3t'}{4}$	$-\frac{h}{2}$	0	$\frac{2\pi^2 h}{t'^2}$
t'	0	$\frac{\pi h}{t}$	0

Letter from Mr. J. A. Longridge to Captain Galton on Paying-out Apparatus for Submarine Telegraph Cables.

Captain Galton, R.E., Board of Trade.

Dear Sir,

I send you herewith sketch of the paying-out apparatus I showed you this morning.

The object is twofold:

1st. To avoid the severe strains caused by the friction of heavy rotatory machinery when the vessel pitches;

2nd. To reduce *ad libitum* the injurious normal pressure of the cable upon the paying-out machine.

1st. This is accomplished in methods 1 and 2 by substituting a sliding motion of the cable over a surface, the friction of the cable being the retarding force instead of the friction of breaks; and in the third method by so arranging the cable over revolving drums that, when the strain exceeds that of the break, the cable slips away, without increasing the velocity of rotation of the machinery. By any one of these methods the inertia of the machinery has no effect on the cable.

2nd. The normal pressure at any point is equal to the tension divided by the radius of curvature. If, now, we take a cable running out with the tension of 30 cwt. over a 5-feet drum, the normal pressure equals $\frac{30}{5}$, $=6$ cwt. per lineal foot, or 56 lbs per lineal inch; an amount which, if the gutta-percha be at all soft, would be very likely to injure the insulation; and suppose that, by a sudden pitch of the vessel, the inertia of the wheels caused an extra tension of 20 cwt., which is greatly within the limits of what might happen, this would bring an extra normal pressure on the cable of 38 lbs. per lineal inch, making altogether $56+38 =94$ lbs. per lineal inch. Such a pressure, besides the injury to the core, would prevent the adoption of a system of sliding, because the friction would be so great as to tear off the entire coating.

Now the only means to reduce the normal pressure is to

increase the radius of curvature, to which there is a practical limit if circular drums be used.

Again, with circular drums the normal pressure is greatest where the cable leaves the drum, and least where it enters on it; whereas it ought to be constant throughout. It is therefore evident that by adopting a curve of variable radius we can obtain a constant pressure.

This is the principle of the methods 1 and 2.

METHOD 1.

The curve of constant pressure is a spiral of which the equation has been investigated.

Representing this spiral by the following sketch, the cable would

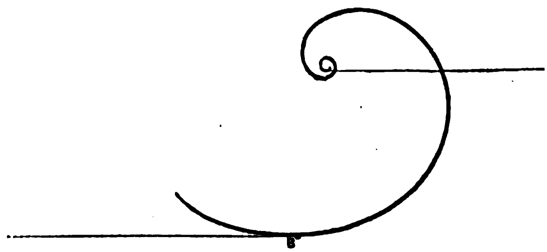


Fig. 1.

enter it at A and leave it at B, and

$$\frac{\text{the tension at A}}{\text{tension at B}} = \frac{\text{Radius and curve at A}}{\text{Radius and curve at B}}.$$

Taking the radius of curvature at B=16 feet and of A=2 feet, the tension at A=$\frac{1}{8}$ of that of B.

The length of curve required depends on the co-efficient of friction, and is of course greater as the latter is less.

The following table gives an example of the action of such a curve, of which the radii are 16 feet and 2 feet respectively.

Let T=tension at leaving the curve.

t=tension at entering the curve.

P=normal pressure on the lineal inch of cable on curve.

p=normal pressure on a 5-feet drum at leaving the drum.

T.	t.	P.	p.
cwt.	cwt.	lbs.	lbs.
32	4	19	120
24	3	$14\frac{1}{2}$	90
16	2	$9\frac{1}{2}$	60
8	1	$4\frac{3}{4}$	30

The next table gives the length of the curves and their horizontal abscissæ (if developed as shown in column 1), corresponding to various co-efficients of friction.

Co-efficient of Friction. f.	Length of Curve. L.	Horizontal Length. l.
	feet.	
$\frac{1}{6}$	84	56
$\frac{1}{5}$	70	47
$\frac{1}{4}$	64	42
$\frac{1}{3}$	42	28

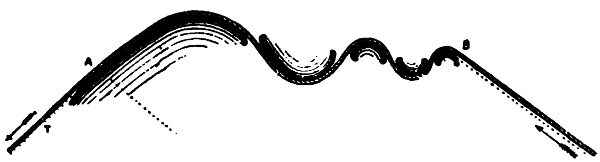


Fig. 2.

The curve may for convenience be divided into sections, and placed either vertically or horizontally along the deck, and so arranged that the cable may be thrown off it in a moment. One or more of the sections may be so arranged as to indicate the tension of the cable.

Method 2.

The curve may be formed by a spiral groove cut in a cone as shown in fig. 2. If this cone be rectangular the curve possesses

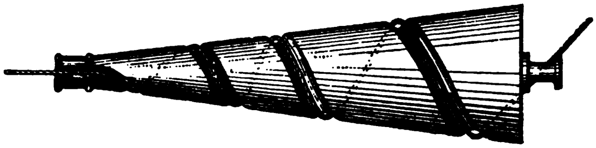


Fig. 3.

the same properties as the curve of equal pressure. The cable enters at the small end of the cone, and makes one, two, or more turns round it, the number of turns depending on the amount of friction required. If the cone be free to revolve, the cable turns it round and runs itself free.

If, on the other hand, it be desired to increase the retarding force, the cone is turned in a reverse direction, and winds more cable on it.

The bearing of the cone is so arranged as to offer no impediment to the escape of the cable when required.

Thus, by this arrangement, the retarding force can be varied at will with the greatest ease.

The cone also takes up much less space than the curve. Its properties in reducing the normal pressure and tension are similar to those exhibited in Table 1, given above, for the curve. It also affords great facilities for indicating the tension.

Method 3.

The cable is here passed between two endless belts passing round drums AA and BB. The standard C is fixed whilst D is moveable, so as to tighten up the belts when required.

The distance apart of the top and bottom drums can be varied at will. The endless belts are formed thus—

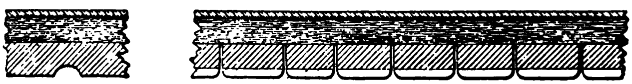


Fig. 4.

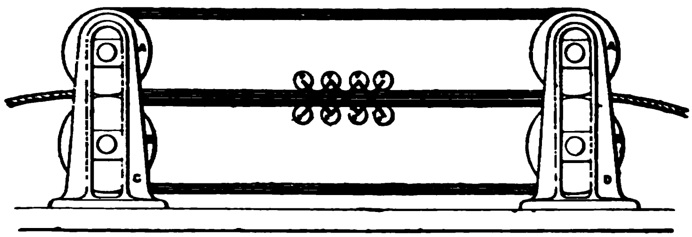


Fig. 5.

and are acted on by small rollers FFF, pressing on the back of each, so connected that the pressure of the two belts on the cable can be varied at will. The cable can be thrown off the machine and put on again in a moment, E is a break-wheel; and F the shaft-end, which may be connected to a small steam-engine for hauling in.

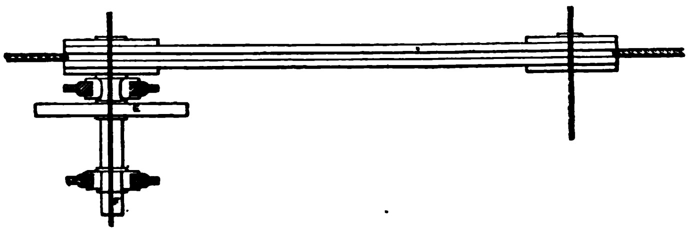


Fig. 6.

Thus it will be seen that the cable runs out with the belts at a tension regulated by a break-wheel, and the increased strain due to inertia is avoided by the cable slipping through the belts when the tension exceeds that due to the friction imposed by the rollers FFF, variable at will.

This machine is peculiarly applicable to picking up cables.

I remain, yours faithfully,

JAMES A. LONGRIDGE.

18, Abingdon Street, Westminster,
28th November, 1859.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. V. 1876. Nos. 15 and 16.

The Forty-eighth Ordinary General Meeting [a portion of the proceedings of which was published in the previous number] was held on Wednesday, the 26th of April, 1876, Mr. C. V. WALKER, F.R.S. President, in the Chair, when the following paper was read:—

ON CLAMOND'S THERMO-ELECTRIC BATTERY.

By Mr. LATIMER CLARK, M.I.C.E.

The thermo-electric batteries recently introduced by M. Clamond of Paris have proved so successful and the demand for them has already been so considerable that I have thought a paper on their construction and application might prove interesting to the members of the Society. M. Clamond has been many years working at the subject of thermo-electricity with a view to bringing it into practical use, and in 1868 he patented his first thermo-pile, in which he employed a sulphuret of lead, in the form of native galena, and iron.

He was subsequently led to abandon this mode of construction in favour of his present system, which he has brought to great perfection, and for which he obtained letters patent in 1874. In the same year the "Thermo-Electric Generator Company" was formed for the purpose of working his patents, with offices at 86, Boulevard de Courcelles, Paris, and a London manufactory at 27, New Street, Cloth Fair. The thermo-piles are extensively used in Paris by the

Government, and by the great electro-plating houses of Messieurs Goupil, Christofle, and other industrial institutions.

In 1875 M. Clamond received the grand medal of the Société d'Encouragement pour l'Industrie Nationale in recognition of the value of his inventions.

Thermo-electric tension (and I use the word tension throughout this paper in its ordinary sense, as signifying difference of potential) is produced whenever two dissimilar metals are placed in contact and heated at their point of junction, and if the cool extremities of the metals are connected together, either directly or by a conducting wire, we have a thermo-electric current, which will continue to flow as long as the difference of temperature is maintained. The metals usually employed to produce this effect are antimony and bismuth, as they exhibit this property in a very high degree, but other metals or alloys may be used with equal or greater advantage.

Markus, in Germany, devised in 1865 a powerful form of battery in which the positive elements were composed of 10 parts copper, 6 zinc, and 6 nickel, and the negative elements of 12 parts antimony, 5 zinc, and 5 bismuth. In another form of his battery he employed for the positive metal 65 parts copper and 31 zinc, and for the negative 12 parts of antimony and 5 of zinc or German silver.

By the term "positive" I mean that a positive current passes through the heated junction from the one metal to the other as in fig. 1.

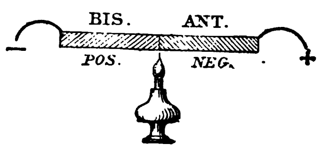


Fig. 1.

The terms here employed are those adopted by electricians, and they are similar to those used in regard to voltaic couples—for example, that metal (zinc) which causes a positive current to flow through the liquid to the other metal is called the electro-positive metal, and similarly with the thermo-electric metals. In each case the wire attached to the positive metal

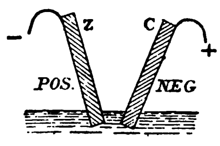


Fig. 2.

gives a negative current when joined up in external circuit, and *vice versâ.*

Professor Dove employed iron and platinum soldered together in alternate lengths, and the whole wound on a cylinder of such diameter as to bring all the iron-platinum junctions on one side of the cylinder and all the platinum-iron junctions on the other. Bunsen used copper pyrites, or pyrolusite with copper; 10 of these elements were equal to a Daniell's cell. Stefan employed sulphide of lead and copper pyrites; 5½ of these elements were said to equal a Daniell.

Farmer in America used Markus metal and German silver, but failed to secure a good permanent connection.

Piles of iron and German silver have been made, and are very efficient, but it requires about 700 elements to give a tension of one volt.

Various other forms have been devised and used by different experimenters, but almost the only practical application of them has hitherto been that made by Nobili and Melloni, who employed them in their magnificent experiments on radiant heat, and they still offer by far the most delicate means of measuring minute differences of temperature at present known—in fact their sensibility is such that even the heat of the fixed stars has been made sensible by their agency.

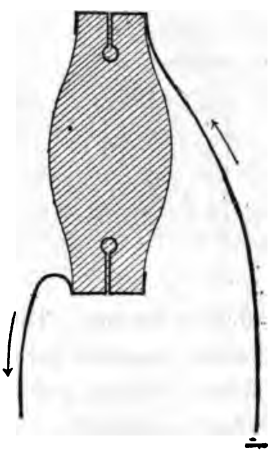


Fig. 3.

The mixture employed by M. Clamond consists of an alloy of two parts of antimony and one of zinc for the negative metal, and for the positive element he employs ordinary tinned sheet-iron—the current flowing through the hot junction from the iron to the alloy. The combination is one of great power. Each element consists of a flat bar of the alloy from 2 inches to 2¾ inches in length and from ⅝ths to 1 inch in thickness. Their form is shown by fig. 3, by which it will be seen that looked at in

plan they are spindle-shaped, or broader in the middle than at the ends.

The following are the three sizes most generally used:—

	Length.	Breadth at ends.	Breadth in middle.	Thickness.
	Inches.	Inches.	Inches.	Inches.
For tension	2	$\frac{1}{2}$	$\frac{7}{8}$	$\frac{3}{8}$
For quantity	$2\frac{3}{8}$	$\frac{5}{8}$	$\frac{7}{8}$	$\frac{9}{16}$
Ditto	$2\frac{3}{4}$	$\frac{5}{8}$	$\frac{7}{8}$	$1\frac{1}{4}$

The sheet tin is stamped out in the form shown in fig. 4; the narrow portion is then bent into the forms shown, in which state they are ready for being fixed in a mould. The melted alloy is poured in, and before it has cooled the mould is opened and the bars removed with the lugs securely cast into them. The mould is heated nearly to the melting point of the alloy, and 10 or 12 bars are cast at one time—a little zinc is added from time to time to make up for the loss by volatilization. The alloy melts at a temperature of about 800° Fahrenheit; it expands considerably in cooling. The more frequently the alloy is recast the more perfect becomes the mixture, so that old piles can be reconverted with advantage and with little loss beyond that of the labour. The alloy is extremely weak and brittle and easily broken by a gentle blow—in fact it is scarcely stronger than loaf sugar.

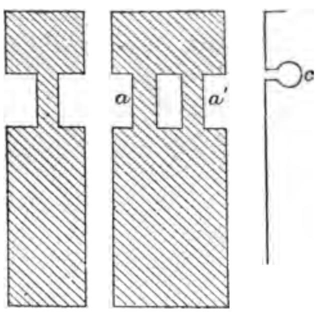


Fig. 4.

The tin lugs are bent into form, and the bars are arranged in a radial manner round a temporary brass cylinder, as shown in fig. 5, a thin slip of mica being inserted between the tin lug and the alloy to prevent contact except at the junction. The number of radial bars varies with the size of the pile, but for the usual sizes eight or ten are employed. As fast as the bars are laid in

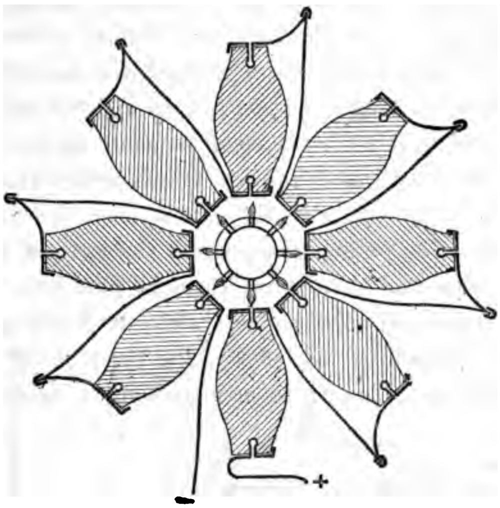


Fig. 5.

position they are secured by a paste or cement formed of powdered asbestos and soluble glass or solution of silicate of potass; flat rings are also formed of the same composition, which possesses considerable tenacity when dry, and as soon as one circle of bars is completed a ring of the dry asbestos cement is placed upon it, and another circle of elements is built upon this, and so on until the whole battery is formed. Cast-iron frames are then placed at top and bottom of the pile and drawn together by screws and rods so as to consolidate the whole, and in this condition the pile is allowed to dry and harden. Looked at from the inside the faces of the elements form a perfect cylinder, within which the gas is burned. The inner face of each element is protected from excessive heat by a tin strip or cap of tin bent round it before it is imbedded in the cement; the projecting strips of tin from the opposite ends of each pair of elements are brought together and soldered with a blowpipe and soft solder. The respective rings are similarly connected, and the whole pile is complete except as regards the heating arrangements. The positive pole of these piles is always placed at the top. Cummings was the first to use this stellar arrangement of the couples.

The pile is usually heated by gas mixed with air on the Bunsen principle; the gas is introduced at the bottom of a tube of earthenware, which is closed at top, and is pierced with a number of small holes throughout its length, corresponding approximately in number and position with the number of elements employed. Before entering this tube the gas is allowed to mix with a regulated proportion of air by an orifice in the supply tube, the size of which can be adjusted; the mixed gases escape through the holes in the earthenware tube and there burn in small blue jets, the annular space between the gas tube and the elements forming a chimney to which air is admitted at bottom, the products of combustion escaping at the top. The whole arrangement is shown in section in fig. 6.

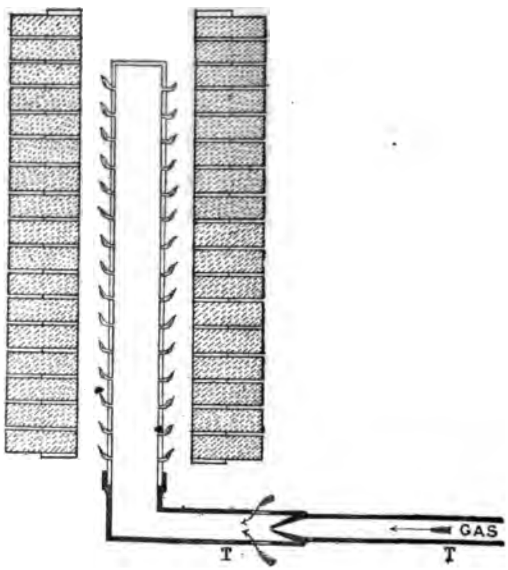


Fig. 6.

When the combustion is going on properly the earthenware tube appears of a uniform dull cherry-red colour throughout, and none of the jets should impinge on the side of the pile. By burning an excess of gas it is possible to overheat the pile and injure the connection of the elements, or even to melt the ends of them—in fact

they require careful and intelligent usage. When in good work the temperature of the inner ends of the bars is about 400° Fahrenheit, and that of the outer ends about 200° Fahrenheit.

In order to prevent injury from overheating and to diminish the consumption of gas, M. Clamond has recently introduced a new form of combustion chamber by which he obtains very great advantages.

This form is shown in fig. 7. The mixture of air and gas is burnt in a perforated earthenware tube as before described, but instead of extending the whole height of the battery it only extends to about one-half of its height. The earthenware tube is

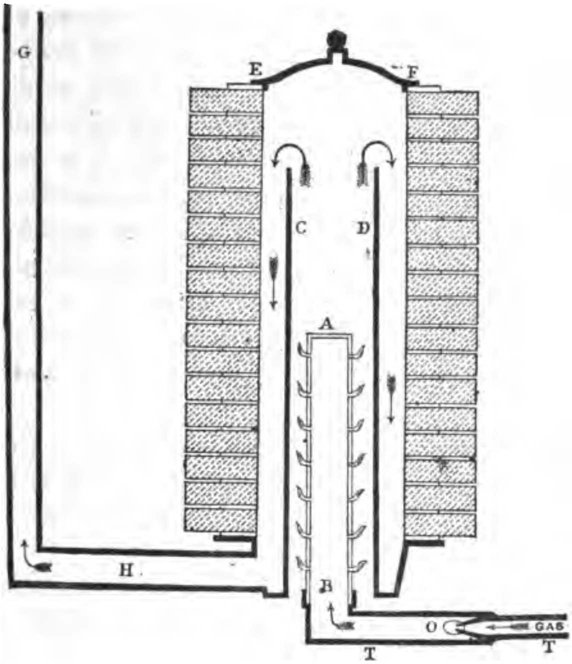


Fig. 7.

surrounded by an iron tube of larger diameter, which extends nearly to the top of the battery, and is open at the top—outside this iron tube and at some distance from it are arranged the elements in the usual manner. A moveable cover fits closely over the top of the pile, and a chimney is connected to the bottom of the

pile leading off from the annular space between the iron tube and the interior faces of the elements. The air enters at the bottom of the iron tube, and the heated gases passing up the tube curl over at the top and descend on its outside, escaping eventually by the chimney. The elements are heated partly by radiation from the iron tube, and partly by the hot gases which pass outside the tube downwards towards the chimney. By this arrangement not only is great economy of gas effected, the consumption as I am informed being reduced by one-half, but the great advantage is obtained that the jets of gas can never impinge directly on the elements, and it is thus scarcely possible to injure the connections by overheating. In the event of a bad connection occurring, it is easy to find out the imperfect element and throw it out of use by short-circuiting it over with a piece of wire, and the makers have no difficulty in cutting out a defective element and replacing it by a sound one.

As the pressure of gas is so liable to vary, it is necessary to employ a small gas regulator with each machine, and the kind they find best is the ordinary dry form, such as those manufactured by Sugg of Oxford Street and other makers. For the ordinary thermo-pile with small-sized elements, the consumpton of gas is in a general way about one foot of gas per hour for each volt of tension, and any ordinary gas-tap will supply gas enough to heat a powerful pile.

Coke and charcoal have also been employed as a source of heat with very great economy and success; in fact there are many countries and places where gas would not be procurable, but where charcoal or coke could be readily obtained.

The general form of the coke-pile is shown in figs. 8 and 9. It consists of the usual surrounding ring of elements, with a cast-iron cylinder within it for the combustion of the coke, a closed air space of two or three inches being left between the iron and the elements; a short chimney at top carries off the products of combustion, and is provided with a damper to regulate the draught. The coke-piles are sometimes made of oval form and of very large size, occupying a space of 6 feet by 4 feet or more. A coke-pile with 400 large elements has a tension equal to 20 Daniell's cells with an

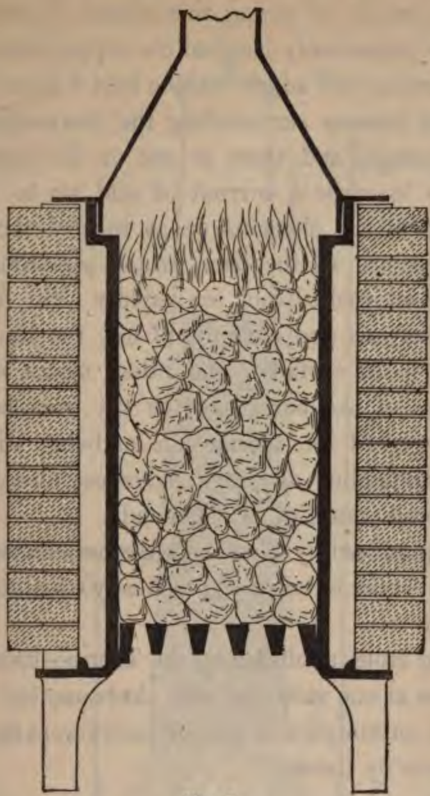


Fig. 8.

internal resistance of only 4 ohms, and its consumption is very steady—two pounds of coke per hour. The recent improvements in the form of the gas-pile will, however, inevitably lead to similar improvements in the construction of the coke and charcoal piles. Charcoal is a very excellent and convenient source of heat. Both the coke and gas piles give a very steady and uniform current, but it takes 15 and 20 minutes after a pile is lighted before it acquires a working tension.

I am not aware that paraffin oil has been successfully used as a source of heat, although it might probably be employed with advantage in some cases. The ordinary paraffin lamp used with Thomson's galvanometer would supply a considerable electromotive force, although the elements should be smaller than those at present in

use. I do not despair of seeing this source of heat utilised, and those who have incautiously handled the copper chimneys of those lamps when burning will admit that the heat is by no means inconsiderable. Iron screens surrounding the thermo-piles have been used with advantage, and there is one on the table before you. Their object is to cause a current of cold air to pass over the exterior surfaces of the elements so as to cool them. They also prevent the fracture of the bars, which sometimes takes place when the pile is allowed to cool too rapidly. So far as I know, the subject of screens and currents of air, the use of water, the evaporation of moist surfaces, and other means of cooling the external surfaces of the bars, have as yet been but imperfectly studied, and I regard the thermo-pile as being still quite in its infancy, notwithstanding the great advances already effected by the skill and perseverance of M. Clamond.

The tension produced by Clamond's thermo-elements is such that each 20 elements may be taken as practically equal to one Daniell's cell, or one volt.

The following table, published by the Thermo-Electric Generator Company, gives at one view the size, electromotive force, internal resistance, and consumption of fuel of the different forms of thermo-pile usually made by them.

GAS-PILES.

	Size.	Electromotive force.	Internal resistance.	Consumption of gas. (Approximate cubic feet.)
	40 bar	2 volts	1·0 ohms	—
For tension ...	60 "	3 "	1·5 "	3
	120 "	6 "	3·0 "	5
	150 "	7 "	3·75 "	6
	240 "	12 "	6·0 "	9
	380 "	19 "	6·75 "	23
	680 "	34 "	12·25 "	37
For quantity ...	50 "	2·5 "	·25 "	6
	60 "	3 "	·6 "	6
	100 "	5 "	1·0 "	9
	200 "	10 "	2·0 "	23
	400 "	20 "	4·0 "	37

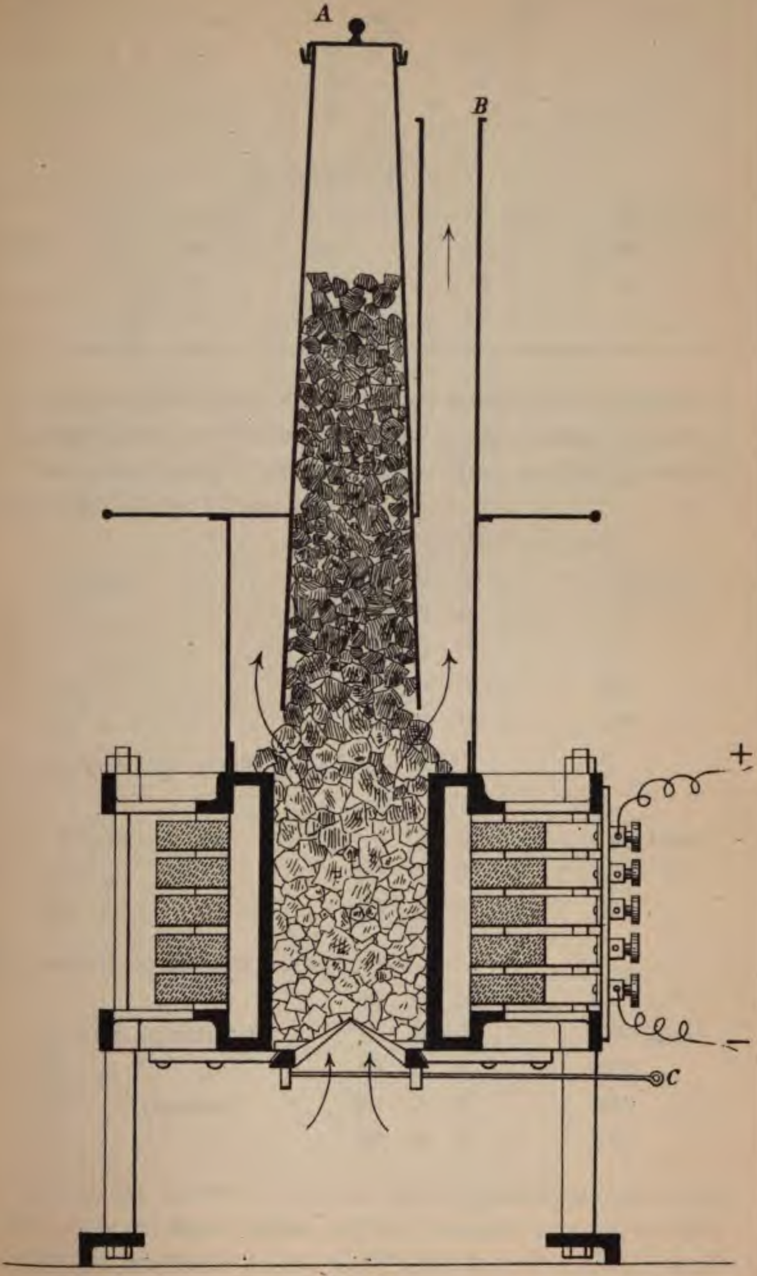


Fig. 9.

COKE-PILES.

For tension ...	340 bars	17 volts	5·75 ohms	1·5 pounds
	680 „	34 „	12·25 „	2·0 „
For quantity ...	190 „	10 „	2·0 „	1·5 „
	400 „	20 „	4·0 „	2·0 „

CHARCOAL-PILE.

For tension ...	100 bars	5·0 volts	1·0 ohms	·33 pounds
	190 „	8·5 „	3·4 „	·50 „
For quantity ...	60 „	3·0 „	·6 „	·33 „
	100 „	5·0 „	1·0 „	·50 „

For the benefit of those who have not access to the circulars of the Thermo-Generator Company, it may be interesting to give a few of the prices charged for these piles. They are for the tension or small-sized bar as follows:—

	£	s.	d.	
2 volts	3	0	0	40 bars
3 „	4	0	0	60 „
6 „	6	10	0	120 „
12 „	13	0	0	240 „
34 „	32	0	0	680 „

For the quantity or large-sized bars:—

	£	s.	d.	
3 volts	8	0	0	60 bars
10 „	20	0	0	200 „
20 „	32	0	0	400 „

The coke-piles are charged as follows:—

	£	s.	d.	
10 volts (quantity)	20	0	0	190 bars
20 „ „	32	0	0	400 „
17 „ (tension)	20	0	0	340 „
34 „ „	32	0	0	680 „

I have myself verified in several cases the tensions and resistances given in the above tables and find them to be very correctly stated. The resistance of the piles when heated is rather more

than 25 per cent. greater than when cold; the resistance given in the above table is that of the piles in their hot state.

A thermo-pile of 144 bars is usually arranged in 18 successive layers, each consisting of eight elements, and, as some of these are more fully exposed to the heat than others, the tension of the several layers varies accordingly. The following is one among many measurements of the different tensions commencing at the top—it applies to the ordinary form of pile:—

1st layer of 8 bars	. . . .	·40 volts
2nd "	. . . .	·49 "
3rd "	. . . .	·52 "
4th "	. . . .	·51 "
5th "	. . . .	·50 "
6th "	. . . .	·49 "
7th "	. . . .	·49 "
8th "	. . . .	·56 "
9th "	. . . .	·50 "
10th "	. . . .	·51 "
11th "	. . . .	·52 "
12th "	. . . .	·52 "
13th "	. . . .	·55 "
14th "	. . . .	·55 "
15th "	. . . .	·56 "
16th "	. . . .	·51 "
17th "	. . . .	·43 "
18th "	. . . .	·26 "
		8·87 volts

It is thus seen that the lowest layer does not give half the tension of the middle layers, and that the upper layer is also weaker than those beneath it. When the pile is first heated the upper bars give the greater tension.

The tension of the pile as a whole was 8·83 volts, and its resistance when hot 3·5 ohms.

The pile takes about one hour to attain its full potential, as shown by the following experiment on a pile of 240 bars:—

Minutes.	Acquired tension.
5	2 volts
10	4 ,,
15	4·75 ,,
20	5·50 ,,
25	6·50 ,,
30	7·25 ,,
40	8·16 ,,
50	8·80 ,,
60	9·0 ,,
70	9·50 ,, (full tension)

If a pile be short-circuited, the powerful current flowing through it tends to cool the interior junctions and to heat the exterior ones, from causes which will be referred to later on. The result is that the temperature of the opposite ends tends to become equalised, and the tension of the pile is temporarily lowered. Thus the above pile, short-circuited by a resistance equal to its own, viz. 3·5 ohms, has its tension soon reduced from 8·5 volts to 8·3 volts, or less. In a similar way the thermo-pile may be "polarised," as it is somewhat inaccurately termed, by passing the current from a Grove's or Daniell's battery through it, the result being to heat one series of junctions and to cool the alternate ones, thus leaving the pile in a condition to give out a very sensible current.

The thermo-piles are so sensitive that it is difficult to find one in such a state of equilibrium as to give no current in either direction; if it be so the passing of the hand into the interior, or the breathing into it, or the approach of the body, immediately sets up a considerable action, and while they are in action the effect of a distant fire or the opening of a window produces a sensible effect on their potential. It becomes important, therefore, when two or more piles are employed to keep them as far apart as possible and to allow them free radiation.

Among the uses to which thermo-piles may be advantageously employed is that of electro-deposition, and they are already much used for electro-plating. The data for calculating the quantity of copper or silver that may be so deposited are furnished by the table already given, remembering that a tension of one volt or even one-

fourth of a volt is sufficient for the deposition of copper. A battery of 375 elements, having a resistance of 4·5 ohms and a tension of 14·6 volts, was found in practice to deposit about 180 grains of copper per hour, but then the tension was sufficient to have created a similar deposit in several successive cells, so that no practical results have yet been obtained or made known which would at all compare with the theoretical possibilities of the case.

The specimen of copper produced is one-half of a plate 6 inches square, which was deposited by 20 bars in 23 hours; the potential was half a volt and the internal resistance ·025 ohm; it is very tough and excellent.

The mechanical power obtainable from these elements by electro-magnetic engines has not yet been put to any practical use. It is certainly given out in a very convenient form, but theoretical considerations abundantly show that it cannot be regarded as ever likely to form an economical source of power. With a particular electro-magnetic engine of ordinary form the lifting power of 90 large-sized bars, consuming 10 cubic feet of gas per hour, was equal to 40 lbs. raised one foot high per minute. A coke-pile burning 2 lbs. of coke per hour would, with this machine, lift 9,000 lbs. one foot high per hour, at a cost of less than one halfpenny, and this without employing the whole of the tension at command. The electric light furnished by these batteries is very powerful and constant, but the tension necessary to produce a good light is so considerable that a very large number of elements has to be employed, and I have not been able to obtain any information as to their practical use for this purpose. I hope that in the discussion which may follow this paper some such information on both the above points may be given.

At this point, the usual hour for adjournment having already been exceeded, it was announced that the reading of the concluding portion of the paper would be carried over to the next meeting.

The Forty-ninth Ordinary General Meeting was held on Thursday, the 11th May, 1876, Mr. C. V. Walker, F.R.S., President, in the Chair.

A communication [the text and illustrations accompanying which will be found at page 257, Nos. XII. and XIV., vol. v.] from Mr. Willoughby Smith on a new form of gutta-percha joint for subterranean telegraph wires was read.

Mr. Latimer Clark said: Although there is to be no discussion upon this communication, I cannot help remarking that this joint is due to one of the discussions which took place in this room, and it shows how much good it does; it shows the practical benefit that is derived from communications and discussions.

The President:—It is my pleasing duty to call your attention to, and to ask your acceptance of, a portrait of Dr. Siemens, and also to invite your vote of thanks to Major Bolton and Major Webber, founders of the Society, for having presented to the Society so exceedingly well executed a portrait as that which you see before you of our first President, a gentleman whom we all greatly respect, and who holds a very high position in science. You will I am sure agree with me that it is a very good likeness of Dr. Siemens. It has been painted by a young lady—Miss Thomas—whose works are generally seen at the exhibitions of the Royal Academy. I beg to propose from the chair that the cordial thanks of the Society be given to the gentlemen who have presented us with this portrait of our first President.

The vote of thanks was passed by acclamation.

The President having announced that the Council had passed Mr. James Draper Bishop from the class of Associates to that of Members,

Mr. Latimer Clark then read the concluding portion of his paper on "M. Clamond's Thermo-Electric Pile."

In the paper that I had the honour of reading to the Society at

the last meeting, I described the general construction of Clamond's thermo-piles, and, had time permitted, I had intended to illustrate their action by making a few measurements of the piles while in work. As the postponement to this evening gives us a little more time, I now propose to review some of the principal facts connected with thermo-electricity, and I hope it will not be found uninstructive if I illustrate some of the principal phenomena by actual experiment.

Seebeck was, I believe, the first to observe the thermo-electric action in 1821 or 1822. The apparatus he used was almost identical with the one you now see on the table.

It consisted of two different metals, antimony and bismuth being found the most effective, soldered at their extremities, so as to form a rectangular frame, within which was suspended a magnetised needle; as soon as either junction was heated a current was produced through the bars, and the needle tended to stand at right angles to the bars.

Professor Cumming made a very careful investigation of the phenomena, and succeeded in obtaining magnetic rotation by means of the current.

I have here a piece of iron and a piece of platinum twisted together, and connected with the galvanometer, which is at a distance of more than twenty feet; but you perceive that the warmth of the fingers is sufficient to produce a distinct current—a piece of iron and copper, or zinc and iron, does the same, and when I employ pieces of bismuth and antimony the warmth of the hand causes the spot of light to pass off the scale and traverse a space of 15 or 20 feet along the wall. I may here remark that the range of the scale is 25 feet, and the distance of the galvanometer from the scale 40 feet.

The galvanometer is wound with about 1,150 turns of No. 22 wire, in two parallel circuits, and has a resistance of four ohms. The galvanometer is at a distance of about twenty feet from the lecture table. The ordinary limelight is used for illumination.

I have here one of Nobilis' thermo-piles, consisting of sixteen very small bars of antimony and bismuth, the whole occupying a space of half a cubic inch; this is so sensitive that the mere approach of

the hand causes a current to appear, and a gentle touch of the finger causes a violent deflection of the galvanometer.

It is of course indifferent whether one of the junctions be heated or the other cooled. Thus, if I pour a few drops of ether on the pile, the deflection is equally powerful in the opposite direction.

As I have before remarked, this form of pile, with a larger number of pairs and with suitable polished reflectors and screens, forms the most sensitive heat-measurer known; if made in a circular form the heat of the smallest insect would be easily shown.

It was soon found that currents were produced not only by dissimilar metals but by the same metal in different states of crystallisation, or of hardness, which alters the molecular condition of the metal and affects its thermo-electric properties. Thus, hammering, or twisting, or stretching the metal, produces a permanent effect. Sir William Thomson, who has diligently investigated this branch of electrical science, as well as others, showed, in 1856, that the mere temporary stretching of a wire by weights or compressing it by force produces a change in the thermo-electric quality of the metal, which disappears as soon as the stress is removed. Magnetisation of a piece of iron or steel produces the same effect. The influence of molecular arrangement is in some cases very powerful. Thus in bismuth a crystal, connected by its side to lead, gives, under certain conditions, a tension of 45 microvolts; if connected by its end the power rises to 65; and if the same metal be pressed hot through a die, so as to form a kind of hardened wire, its force increases to 97.

Metals are not the only substances which produce thermo-electric effects. Most other conducting substances do the same; thus the sulphides of lead (galena), iron pyrites, copper pyrites, and the sulphides of other metals, the native oxide of manganese (pyrolusite), phosphorus, carbon, and other substances, produce powerful effects, and have been successfully employed in making thermo-piles. Selenium is in fact by far the most thermo-negative substance known, being more than thirty times as powerful as antimony.

There is another source of thermo-electric power, viz. that produced when two pieces of metal are heated to different temperatures and connected by a fused salt capable of conducting

electricity; it seems to be indifferent what metal is used or what is the nature of the salt. The current is always from the hotter metal through the salt to the cooler metal.

There is also a thermo-electric effect produced when certain crystals are heated—the best known of these is tourmaline, which, when heated, exhibits electricity of high tension at its ends—the electricity being positive at one end and negative at the other; even broken fragments of the crystal have the same property.

We have hitherto spoken chiefly of the effect produced by a contact of dissimilar metals, but there is another way of producing a thermo current. When two pieces of the same metal are placed in contact, one hot and the other cold, a considerable thermo-electric current is produced. I have here two pieces of iron-wire. I heat one in the spirit lamp and place it in contact with the other; a current passes through the heated junction from the hot metal to the cold; if I heat the other piece of metal the current is reversed.

Platinum, copper, zinc, and other metals produce the same result. Hot and cold mercury produce no such effect.

Sir William Thomson discovered the singular fact, that when a metal circuit is heated at one spot, and a current is passed through it, the heat is conveyed by the current in a direction dependent on the nature of the metal. With iron or platinum the heat is carried by the positive current from hot to cold, with copper and other metals in the reverse direction.

Peltier, in 1834, had discovered a somewhat analogous effect. His theory was, that, when a thermo current was formed in a circuit of bismuth and antimony, there was an absorption of heat at the surfaces of contact at the warmer junction, and an evolution of heat at the colder junction, and that the same result was obtained if a current from any exterior source were passed through such a circuit; this fact is easily verified by experiment. I have in this small box an arrangement by which I can send a current from a single Smee's cell through a circuit of bismuth and antimony; in immediate juxtaposition with this is another similar circuit, and the wires from this last circuit are connected to the galvonometer; there is no electric connection between them, as the two are sepa-

rated by a thickness of oiled silk, which is a very good conductor of heat, but not of electricity; the whole is surrounded by cotton wool. When I connect the battery with the first pair, there is at first no effect visible, but in a few moments the heat produced by the passage of the current across the junction of the two metals passes through the oiled silk and causes a thermo current in the second pair. When the current through the first pair is reversed cold is produced, and after a short interval of time its effects are manifested by the production of a current in the opposite direction through the secondary circuit.

Lenz showed that the cold thus produced was sufficient to freeze water if the current was passed between two pieces of metal cooled to about 33° or 34° Fahr., and I would repeat the experiment now but that our time does not permit.

I now exhibit a table of a few of the principal metals, arranged in the order of their thermo-electric powers; the upper elements being the most positive, that is to say, a positive current passes through the heated junction from any metal to any other metal lower down in the list than itself and *vice versa.*

THERMO POSITIVE :—

	Silver and copper = 1.	Microvolts per degree Centigrade.
Galena	— —	
Bismuth	+ 24·96	+ 45 to 65
Mercury	+ 2·52	+ 4·18
Aluminium	+ 1·28	
Lead wire...	+ 1·02	
Tin wire	+ 1·00	— ·1
Copper	+ 1·00	— ·1
Platinum	+ ·72	— ·9
THERMO NEGATIVE :—		
Silver	— —	— 3·0
Graphite	— ·05	
Zinc	— ·21	— 3·7
Iron (piano wire)	— 5·20	— 17·5
Antimony...	— 9·87	— 23 to 26
Antimony 2, Zinc 1	— 22·70	
Tellurium	— 179·80	— 502
Selenium	— 290·00	— 807

The table is extracted from Dr. Matthiesen's paper in the Philosophical Transactions for 1858. Referring to the left-hand column of figures it will be seen that he takes silver as his zero metal, and, calling the effect of a silver-copper pair 1, he measures the electro-motive force of all the other couples by this standard, those above silver being considered thermo-positive metals, and those below it thermo-negative.

The right-hand column is a table calculated from this by Professor Jenkin, showing approximately the thermo-electric force of each couple in microvolts for each degree centigrade. Professor Jenkin, however, takes lead as his zero instead of silver. The tensions in the upper half of the table are therefore positive and those in the lower half are negative, and to obtain the power of any positive and negative couple we must add the two together; thus bismuth and antimony have a force of 45 + 23 as compared with that of silver and copper, or a tension of 68 microvolts for each degree Centigrade.

As these tensions are relatively fixed quantities, it matters not in what order the metals are combined; a compound chain of various pairs of elements gives the same force as the two exterior elements. We may gain our tension by uniting successively bismuth, lead, copper, zinc, iron, and antimony, the intermediate metals being arranged in any order whatever, or by taking the bismuth and antimony alone, and in both cases the resultant tension will be the same.

It is rather remarkable in regarding the order of arrangement of the metals in this table that it does not appear to have any relation whatever to the order in which they stand to each other as regards their power of conducting electricity or heat or their relative specific heats, or the order in which they stand to each other as respects their voltaic forces. The old familiar copper and zinc combination has very little thermo-electric power, and the same is true of zinc with silver, platinum, and carbon. On the other hand, bismuth and antimony, which have little voltaic power, stand at the extremes of the scale. Selenium is singularly negative, and galena and some of the sulphides very positive.

If we seek through the table for useful thermo-electric combina-

tions, we must remember that our elements ought to be very good conductors of electricity and very bad conductors of heat: they should also be cheap.

In the positive list bismuth, which stands at the head, is at present a very expensive metal, and is also easily fused. German silver and copper appear the best metals available. In the negative series we find iron and antimony, and yet it is singular to find M. Clamond employing iron as his thermo-positive metal; there is, however, a reason for this which will appear hereafter. It is remarkable that the alloy of antimony and zinc which M. Clamond used is so much more powerful than either of the elements alone, being two and a-half times more efficient than antimony. This combination was, I believe, first made by Rollman, and is described in Poggendorff's Annalen, 83, 84, and 89. Marcus (or Markus) employed a somewhat similar combination. The thermo-electric force is said to be the greatest when the two metals are combined in the proportion of their chemical equivalents, but the increased resistance of the alloy neutralises the electrical advantage thus obtainable.

The thermo-electric relations given in the above table are roughly true for ordinary temperatures. For every increase or decrease of temperature some change of power occurs, and at last the very order of arrangement of the elements is changed. For example, at 300° Centigrade iron stands above copper, silver, and zinc. Sir William Thomson, who investigated the subject fully as long ago as 1856, gives several such tables for different temperatures, and his admirable paper on the subject will be found in the Philosophical Transactions for that year.

The fact of thermo-electric inversion was first observed as early as 1823 by Professor Cumming, and it occurs to a more marked extent with iron than with other metals on account of the great change in thermo-electric force which that metal undergoes at different temperatures.

I have here a piece of zinc with a piece of iron wire twisted around it; on applying heat to the junction a current passes from the zinc to the iron, and as the temperature rises the current increases in force; presently, however, it attains a maximum, and

as the heat further increases the force diminishes. As we approach the melting point of the zinc we come to a temperature at which there is no thermo-electric action whatever; the needle stands again at zero, as it did before the heat was applied.

But now, as the heat further rises, the direction of the current is reversed, and the iron becomes positive to the zinc, and, if I were to continue applying heat above the melting-point of the zinc, the deflection would be as great on the one side of zero as it was on the other; iron and copper present the same phenomenon.

Now, in order to understand the full meaning of this phenomenon, it is necessary to refer to the diagram No. 10.

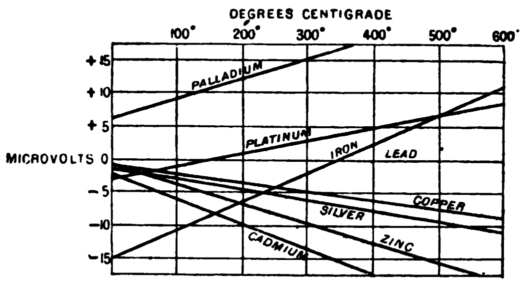


Fig. 10.

This diagram is extracted from Professor Jenkin's work on Electricity and Magnetism, but was first given by Sir William Thomson in his Bakerian Lecture of 1856.* The figures along the top of the diagram represent temperatures ranging from zero, or the freezing point, on the left, up to 600° Centigrade on the right. The figures on the side of the table represent tension expressed in microvolts, and in the upper half of the table the tensions are plus or positive, and in the lower half negative. The zero or middle of the table is occupied by a line representing lead, and it is assumed for the purposes of this diagram that this metal has the same thermo-electrical power at all tensions from 0° to 600° Centigrade. Now this being so, the tensions of the other metals at all temperatures are represented by the diagonal lines which cross the diagram. Thus platinum at 0° Centigrade stands on the left a little

* The diagram referred to will be found at page 178 of Prof. Fleeming Jenkin's work. He has reduced the tensions to absolute measurement.

negative to lead, but at 600° it becomes positive to the extent of nearly ten microvolts; at about 150° it is indifferent to lead, and gives no thermo current. Zinc, on the contrary, is indifferent to lead at some temperatures, a little below freezing not shown on the table, and as it gets hotter it gets more and more negative, and at the temperature of 600° it becomes negative to the extent of nearly twenty microvolts. Iron, which stands among the negative metals in the ordinary list of thermo elements, starts at ordinary temperatures very negative to lead, but at higher temperature becomes very positive to it, and it is this property which, with the high temperature attained in M. Clamond's piles, makes it a more suitable metal for the positive element than any other. The lines for bismuth and antimony are unfortunately not given in Sir William Thomson's diagram. It will be seen that at about 360° Centigrade iron and zinc are indifferent to each other, as we saw also by our last experiment, and this neutral point is one of much interest, for if we were to connect a series of different metals together, and heat each of the junctions to its neutral temperature, we should have a compound pile heated or cooled to various temperatures, and yet without the slightest thermo-electric tension in any part of it.

The following table, prepared by Professor Tait, gives the temperature at which certain metals are neutral to each other.

Iron	+ 357
Tin	+ 45
Brass	+ 27
Lead	—
Zinc	− 32
Copper	− 68
Cadmium	− 69
Aluminium	− 113
Silver	− 115
Palladium	− 181
German Silver	− 314

We will now measure a few simple elements with the junctions heated to 0° Centigrade and to 100° Centigrade respectively, the

junction of each being immersed, the one in melting ice and the other in boiling water.

The apparatus by which I make these measurements is well known to you, and is fully described in the Society's Journal, vol. ii. p. 21, under the name of a Potentiometer, so called because

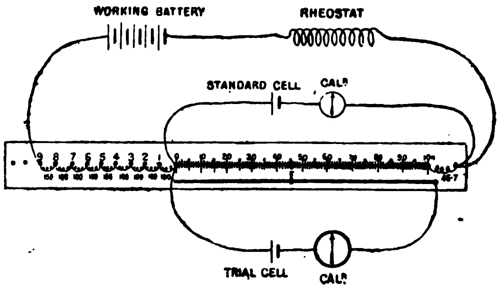


Fig. 11.

it is one of many forms of instrument for measuring differences of potentials (fig. 11). A wire of German silver, whose resistance I can vary at will from 145·6 units to 1456 units, or 14560 units, has a difference of potential of 1·456 volts maintained between its extremities, by connecting them to a Clark's standard cell, and maintaining that exact potential by the agency of a large thermo battery; each unit therefore represents $\frac{1}{100}$ of a volt when the resistance is 145·6 units, $\frac{1}{1000}$ when it is 1456 units, and $\frac{1}{10000}$ of a volt when it is made 14560 units; the actual resistance of the wire when the resistance is at the lowest is 9·772 ohms, and when at the highest 977·2 ohms.

On the lower scale the stretched wire enables me to measure any tension up to 1 volt, and the 9 coils on the left, which are each of the same resistance, enable me to add successively 9 volts, so that I can measure at once any tension from $\frac{1}{100}$ volt to 10 volts. On the higher scale I can measure from $\frac{1}{10000}$ volt to $\frac{1}{10}$ volt.

The tensions I obtain are as follows:—

1.	Copper and iron - - - -	·0006 volts
2.	Platinum and iron - - -	·0011 ,,
3.	German silver and iron - -	·0020 ,,

4. Antimony and bismuth - - - ·0051 volts
5. Clamond's alloy and iron - - ·0102 ,,

and it is interesting to observe how much higher the alloy of antimony and zinc stands than that of any of the other elements.

(Mr. Clark here proceeded to make several measures of the potential of Clamond's piles by means of the potentiometer, the results of which were shown to be in accordance with those given in the table at pages 330-331.)

I believe that there are about 250 of these piles now in use in England and about 300 in France, besides others in almost every civilised country. I have seen a list of nearly 20 large firms or companies already using them in this country, besides a vast number of foreign Governments and institutions. I have found them extremely convenient for laboratory use, and by utilising the waste heat of the kitchen fires they might, I have no doubt, be advantageously used for domestic purposes.

Mr. W. H. PREECE: I am quite sure I shall be echoing the thoughts of all here present when I say that we are most thankful to Mr. Latimer Clark for having brought before this Society not only such an interesting subject, but for having brought it before us in such a perfect, complete, and successful manner. We have never had anything produced before the Society in such a way before, and we can only hope that it will be the prelude to several such papers. It shows this, that if good men will bring forward good subjects they will always secure good audiences; and I am sure the audience on this, as well as on the last occasion, must be taken as a compliment to Mr. Latimer Clark. The Post Office authorities have had some experience of the practical working of these thermo-electric piles. For some months past we have employed in Telegraph Street five pairs of Clamond's thermo piles in working circuits on what is called the universal battery system, that is, working many circuits from the same set. Three were employed first in working 22 circuits, which were gradually increased to 42; and the two others were employed in working first 20, which were gradually increased to 48, so that really these five piles of thermo-electric batteries were used in working 90 separate and distinct circuits from Telegraph Street. I should mention that the circuits

were all under 100 miles in length. The result was satisfactory, only, I am compelled to say, certain faults developed themselves. In working out a new thing we must expect to meet with faults; faults are simply the lessons of experience, and if we did not meet with faults we should not succeed in advancing and reaching perfection. The result of these faults has been to produce that last improvement which Mr. Latimer Clark explained, where the diminished action of the little jets of gas was prevented from being projected upon the metal itself by the interposition of an iron plate. This new form has not yet been introduced at Telegraph Street, and we hope when we introduce it to be able to work even a greater number of circuits than we do at present. One great merit the system has displayed is this—that from a surface not much larger than this table we have been able to work these 90 circuits and to replace by their means no fewer than 2535 cells. Any apparatus that in these days of high pressure economises space is in itself a great advantage. At the same time we cannot expect a novelty of this kind to be introduced without some demerits either in the shape of expense or other points. At present our experience has not enabled us to arrive at any definite result as to expense. The result is, up to the present, the figures given by Mr. Latimer Clark, are very nearly exact, viz. that these five piles consume 44 feet of gas per hour; and from that it is not difficult to calculate what the cost will be. I will not say more except this: that nobody can watch the results of such experiments as have been brought before us, or the working of such an instrument as this, without feeling that the molecular theory of electricity—that which accounts for the display of electrical effects by the conversion of one energy to another—must receive more acceptance, for we in these experiments have the simple change of the molecular constitution of bodies producing those conditions which determine different electrical effects.

The PRESIDENT: The time for closing our meeting is nearly arrived; but during the few minutes that remain at our disposal we should be glad to hear Mr. Higgins, who, I believe, is not unacquainted with these instruments, should he be disposed to favour us with any remarks. I may add that the paper is so interesting, that the experiments have occupied so long a time — not one

minute too long—and that before we meet again we may expect that the whole subject—the paper itself, and the discussion, as far as we have gone —will be in your hands in print—so that you will have a good and long opportunity between now and November of thinking over many points. We will therefore open the remaining portion of this session of 1876 in November by resuming the discussion.

Mr. HIGGINS: There is not much time left for me to say what I could wish; but I would correct one statement which the President has made, viz. that I am connected with the Thermo-Electric Generator Company. I am not in any way connected with it.

I have estimated the relative cost* of depositing one ounce of copper by some of the batteries most used for that purpose, and find that the thermo-electric piles heated by coke are the most economical; next in order come the bichromate, Smee's, Grove's, gas-heated thermos, and Daniell's. It might be supposed that the Smee's battery, from the simplicity of its action and cheapness of the materials employed, would have been first on the list, but I found, on trial with a battery having a new platinized plate, that the potential during action diminished more than 40 per cent. from its potential, with battery inactive, and from the very rapid rate of increase when the potential was being taken I believe it must diminish about 50 per cent., or to about $\frac{1}{2}$ volt. The quantity of zinc dissolved in an element has no more to do with the quantity of copper the battery is capable of precipitating than the quantity of water evaporated has to do with the power of the steam produced; the chief consideration is the tension or the force.

With a bichromate element having a potential of two volts precipitation of copper can be effected in four successive vessels, while the consumption of zinc is only chemically equivalent to the copper deposited in one vessel. I mention this to show the great adaptability of the thermo piles to electro-metallurgy. The intensity and volume of the current can be varied at will to suit the surfaces and solution employed. Although the bichromate battery generates electricity more cheaply than the gas-heated thermo piles, its tension cannot conveniently be reduced to that just sufficient to precipi-

* See table at pages 348-349.

tate copper, and a system of vessels arranged to utilize the current up to the extent of its capabilities would be difficult to manage.

The applications of this pile are very numerous. The force of a small pile working a motor day and night could keep the weights of a large clock always wound up. Permanent magnets for the removal of iron from brass filings can be made and rotated by the same battery. The encrustation of steam-generating boilers may be to some extent prevented by the current from a small pile built around the flue or any other hot place. A small circle of ten pairs placed above the flame of a signal lamp would indicate the state of affairs to a distant signal box.

The two largest piles we have had at work had an electromotive force equal to about 120 volts. The electric arc from these two piles was about ⅜-inch long, and could be maintained for any length of time. One small coke pile equal to 20 volts, and offering four ohms internal resistance, was kept at work for some months, and performed most satisfactorily. It is very easily maintained at its full power if the grate be stirred about once every two or three hours. A coke pile this size, built up with small bars, would contain 1200, and be equal in electromotive force to 60 telegraphic Daniell's, but twelve times that number would have to be joined up quantitatively to make the internal resistance equal. Such a pile, therefore, could do the work of at least 720 telegraphic Daniell's. Mr. Preece shows that these piles can be put to more work than would be estimated from their power as compared with ordinary batteries, I may then safely say that such a pile would do the work of 1000 cells at a cost of about 4*d.* per day of twenty-four hours.

COST OF DEPOSITING ONE OUNCE OF COPPER BY VARIOUS PILES.

Battery.	Potential taken as.	Cost in farthings.
Daniell's . . .	1·0	1·9418
Bichromate . .	2·0	·9823
Grove's . . .	1·8	1·6145
Smee's . . .	·5	1·1750
Thermo, heated by coke		·51
Do. heated by gas		1·72

Prices taken as—

Bichromate	6*d.* per lb.	
Nitric	10*d.* ,,	(fuming).
Sulp. of Copper	$3\frac{1}{2}$*d.* ,,	
Sulp. Acid	1*d.* ,,	
Zinc	$3\frac{1}{2}$*d.* ,,	

On the motion of the President a cordial vote of thanks was passed to Mr. Latimer Clark for his paper, and for the interesting illustrations by which it had been accompanied.

Mr. LATIMER CLARK: I have only to thank you for the kind manner in which you have received my paper, and I am glad if it has been of any use to the Society. I may add that the apparatus I have used is at the service of any member who may desire to employ it for his own purposes for a short time. I am aware that it is rather a costly arrangement for a single paper, but in doing so my object has been that it might to some extent be at the service of the members.

The following Candidates were then balloted for, and declared duly elected:—

As MEMBER:—

R. Sawer.

As ASSOCIATES:—

F. Thornton.
W. Downing.
H. Kingsford.

After which the Meeting adjourned.

The Fiftieth Ordinary General Meeting was held on Wednesday, the 22nd November, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The President rose and said—

GENTLEMEN,—I am very pleased again to meet you after our long recess, to meet you again as heretofore under this very hospitable roof. However much you and I may have been resting, and then again working after our respective holidays were ended, the officers of the Society have unquestionably been most diligent —they must have had more work than rest; for they have brought us into the happy position that all arrears of printing up to the present date are worked up, and No. 13 of the Journal of the Society is very nearly ready for issue. I say the printing has been worked up to the present date. It is so, except that it was thought more convenient for Mr. Latimer Clark's paper on the thermo-electric battery, read at the meeting previous to the recess, and the discussion of it which was adjourned till this present meeting, might appear together in the forthcoming number. And I may also state that arrears of another kind have been worked up successfully, I mean the arrears of subscriptions; for I need hardly say that working up the arrears of printing would have been in vain if the Treasurer was not in possession of funds to pay the charges which the printer has against us. I will now call upon the Secretary to read the minutes of the last Ordinary Meeting.

The minutes having been read and confirmed and the list of new candidates read,

The PRESIDENT said: I wish to inform you that the Society has received a very unique, valuable, and interesting document since last we met. It is in the Latin language, and will appear *verbatim* in the Journal of the Society, together with a translation.* It is an impression—not a copy or reprint—of the original communication made by Professor Œrsted relative to the discovery of electro-

* The *verbatim* copy of this document, together with a translation in English, will be found at page 459 of the present number.

magnetism. The possession of this valuable document will add very much to the interest of our library. The Society have also received an autograph of Professor Œrsted himself. I will now call upon Mr. Latimer Clark to give us the benefit of any further observations he may feel disposed to offer in continuation of the remarks upon the thermo-electric pile which he laid before us last Session.

Mr. LATIMER CLARK: I have now only to describe the new form of apparatus which has been introduced to your notice this evening. The improved apparatus on the table is that of Mr. Leonard Wray and Mr. Cecil Wray, the sons of Mr. Leonard Wray, who is well known to many of us in connection with the insulating material known as Wray's Compound, and as having taken an active part in the introduction of this battery into England.

The first improvement is in the manner of casting the bars. They are cast under pressure as they always have been, and the change consists in the use of a small tongue of tinned iron cast down the centre of the bar extending nearly its whole length. This adds materially to the strength, and it is stated that it also decreases the resistance and increases the electromotive force. At any rate it increases the strength in the proportion of about 30 lb. to 50 lb. so that they are not so fragile as before.

The next improvement is in the method of building up the battery. As I described on the last occasion, the bars have hitherto been arranged in circular rings separated by a disc of silicate of potash and asbestos; then another set of bars and another disc and so on. The consequence was that the whole of the weight of the upper bars rested upon the lower ones, and this weight added to the pressure of the screws was sufficient to cause the metal, when nearly melted, to lose its form and ooze out. Mr. Wray now builds up the battery by a number of discs made of burnt clay, pipe clay, or biscuit ware, and between each disc interposes a small triangle of the same material, with metal rods to hold the whole together. The consequence is these discs and triangles when in place sustain the whole pressure, and the thermo bars rest upon them and can be removed and re-arranged when required; by this improvement they are relieved from

pressure and are not so liable to be injured by the heat of the battery.

The third improvement is in the method of heating. Instead of the gas flames impinging directly upon the bars or against the iron cylinder within the thermo-battery they now employ an inner cylinder of earthenware, which forms the centre of the battery, and they build up the bars of metal around the cylinder and in close contact with it, each bar being bedded up against it with asbestos cement. The flame therefore cannot get in contact with them, and they are less liable to be injured by heat, which was the chief cause of difficulty in the older form of pile.

Another advantage is that the heat is more uniformly distributed, and when the gas is extinguished the battery cools much slower. The rapid cooling of the battery after the fire is extinguished often causes fractures when the gas is first lighted and the whole pile is cold; there is moreover a great condensation of moisture, which had been supposed to cause the bars to crack and to injure them by causing oxidation; all these defects are more or less obviated by new construction.

The next improvement is in the gas chamber. Those who use these batteries know that there is sometimes an explosion when you first light them. They now use a safety gas chimney of wire gauze inside the burner, through which the gas passes before it emerges from the burner, and the consequence is you cannot have any explosion.

They have also made an improvement in the regulation of the supply of air as well as of gas; beneath the battery they have an arrangement by which a disc can be lowered or raised so as to adjust the amount of air and insure the most perfect combustion possible.

Lastly, they further regulate the supply of air by little covers of fire-clay placed on the top of the battery. They consist of perforated radial discs. Improvements have also been effected in the coke form of battery, and the novelties introduced are these: they have two doors in the coke pile, by means of which the combustion can be inspected and the vertical bars on the front of the pile can be easily removed and replaced, clinkers too can be easily removed,

whereas formerly it was necessary to empty the pile to get out the clinkers. They also have the same improvement as that introduced in the gas pile, that the bars are supported by clay discs, and any set of bars may be removed or replaced. There is also in the coke pile an improved method of feeding the fire and regulating the combustion. I have no doubt that with all the improvements that time and experience will indicate we shall soon consider thermo-piles indispensable for telegraph purposes as well as for electro-chemical deposition.

Mr. W. H. PREECE : I have very little to add to what I said on the previous occasion when this subject was mooted here. I then mentioned that six of these piles had been supplied to the Post Office department by the Company, and had been used. They were employed on the universal battery plan. At first twenty circuits were attached to the six piles, which were gradually increased by fives, until at last forty-three distinct circuits were working simultaneously from those six piles. Each of these piles successively failed, and every one from the same cause—that named by Mr. Latimer Clark in his paper, viz., the overheating, by which the connection between the two plates was fused or destroyed. The system of introducing an intermediate plate, so as to work by radial heat instead of the direct application of heat, had been applied to some of them, but owing to the collapse of the Company the complete set promised to the department has never been supplied, and the result is that since March last the experiment has been discontinued. I feel, however, bound to say the results given were so satisfactory as to make the use of this battery very promising, and we are in hopes that this Company will recover its position, so as to be enabled to supply those batteries that are required to carry out the experiments. The results we have found in practice corresponded exactly with those stated in the prospectus itself, and also with the measurements of Mr. Latimer Clark. The consumption of gas was 8 feet per hour, and the electrical force and internal resistance agreed with the figures given. When this order is completed, when the batteries are again attached to the circuits, and after they have had satisfactory trial, I shall have great pleasure once more in bringing the matter before the Society.

Mr. Cecil Wray (responding to the President's invitation) said: I do not know that I can say much in addition to the able description you have heard from Mr. Latimer Clark; but there are one or two things I may point out with regard to the increase in the strength of the bars, secured by the adoption of this internal tongue, which may be interesting. I have here some bars that have been built in a pile, which was working some considerable time. They have not been in any way injured by heat or otherwise during that working; and I may also observe that the pile was made of bars of two or three different kinds. To test the relative advantage of this long internal piece, I will break one or two of these bars, and you will have the opportunity of seeing [breaking a bar] that this has one of these long positives in it. You will notice that it does not run right through the bar. You will also notice that the bar generally breaks just at the point at which this long tongue ends, and that the remaining portion is fully supported by the discs. The object of the long tongue is to protect the outer and exposed ends, so that, should the bar get cracked at any time, the current will not cease; and, when broken to that extent, the bar still clings together. In a bar I have here you will see the *connection* which takes place.

This model shows the arrangement of the bars around the tube, against which their inner ends abut and are in contact. They are all built in with clay or other suitable material, so that neither air nor the products of combustion can affect the junctions; consequently, after working a considerable time, there is not the slightest deterioration of the iron positives, such as takes place when they are not so protected. In the larger pile (of the coke machine) the bars are built in vertical, instead of horizontal, sections, so that they can be easily removed in the event of the pile being required to be taken from one place to another; whereas formerly, the coke machine being a large one, it was necessary to take it to pieces entirely to allow of its being removed.

Mr. Leonard Wray, Sen.: I would ask permission to add one or two words upon the great point which has been mentioned by Mr. Latimer Clark. It is as to the absolute uselessness of a machine that is liable to fuse. Unless you can do away with that

liability, and insure that the ends of the bars shall remain uninjured by the heat and the products of combustion, you have a constant source of danger. Sooner or later the bars must come to grief: oxidation *must* take place, and gradually the current ceases. You can never have a good and enduring thermo-electric machine unless that tendency is got over. The open bars—that is, where the inner ends of the bars are exposed, as they have hitherto been —are always subject to that danger, and it is not alone a single danger, because you have the further danger of *damp*, arising from condensation, and likewise the danger arising from the products of combustion. All these act upon the very thin piece of iron, which is simply coated with a little tin, and this tin, as you know, will not stand any large amount of heat, and the consequence is that in a very short space of time the destructive action commences, and when once commenced continues with greater or less rapidity, according to the manner in which the pile is treated after the gas is shut off. My sons have endeavoured to get over this difficulty by the employment of a central cylinder, which may be of varying thickness, according to the metal or alloy used in the bars. As has already been touched upon, there are several advantages in this. It is not alone the protection afforded to the bars, but it is also that this tube soon becomes, by reason of the well-adjusted flame and heat, a perfect glowing mass, and you obtain thereby the great object of *uniformity of heating.* Unless the heat becomes comparatively uniform I need not tell experienced men like yourselves that you get only an inferior current. One tier of bars will give one result and another tier another result, and the consequence is that they all vary. In the paper for which we are so much indebted to Mr. Clark, he has shown clearly enough that, although the difference therein exhibited was comparatively small, it must have been a good machine he experimented upon, for in practice it has been proved that the difference of the current from the several tiers is usually very great; and that arises from the fact of there being no uniformity of internal heating. Two very essential conditions in a machine of this kind are—*uniformity of heat internally*, and *uniformity of cooling externally.* You will never get a perfect machine till you accomplish these two objects,

and the nearer you approach to them the nearer you will approach to a perfect machine. In this case I venture to say that my sons have made a very decided approach towards the first object, that is, in obtaining a uniformity of internal heating. The bars abutting upon this tube have almost the same heat at the bottom as they have at the top, as the tube is one glowing mass; besides this, when you turn off the gas you have only to stop the bottom and the top, and you insure a cooling down gradually. I need not tell you in a machine of this character it is most important to avoid sudden heating and sudden cooling, that great change from the heat we get from a Bunsen burner to the rapid cooling caused by an inrush of cold air is very liable to crack the bars, and when that is the case there is immediate deterioration and consequent destruction. But by *this* system, you observe, the cooling is very gradually effected. This tube takes a considerable time to cool down, more especially as it is closed at the top and bottom. This sudden heating and rapid cooling was no doubt *one* of the causes that brought about those disasters in the machines to which Mr. Preece has alluded. Wherever the inner ends of the bars are unprotected they must inevitably perish.

All the other points I think have been made so plain that I will not trouble you with further reference to them. I have simply spoken on this one point as being really at the root of the whole evil, and if a remedy has been hereby obtained, or even a fair approach to a remedy, I submit that a certain and important improvement has been effected.

Professor Forster: May I ask one question of Mr. Latimer Clark or Mr. Wray with regard to the action of the tongue, which is described as being cast into the bars of this apparatus? I am not sure whether I have quite understood the arrangement of that; if I have, I do not understand how it acts. It appears to me the effect would be to diminish the resistance at the expense of electromotive force, and that the tongue extending from one end towards the other one should have less difference of temperature between the internal end of the tongue and the end at which the other piece of iron is connected than if we had the piece of iron merely stuck on at the other end. If measurements have been made showing what

the action set up is they would be valuable, because the mere increase of current would not be conclusive under the circumstances. It might be the result of a decrease of resistance or of an increase of electromotive force; and it seems possible that an actual diminution of electromotive force might be more than compensated by the diminution of resistance.

Mr. LEONARD WRAY, Jun.: I may say that the bars *increase* in electromotive force when the elongated end, or tongue, is employed, and also that the internal resistance is diminished. That was a result unexpected on our part, and I cannot at present explain it; but it is nevertheless the fact, and I may state, that we have a pile so built that the bars are all exactly equally heated in order that there should be no mistake. While bars made exactly the same way, but *without* the tongue, gave ·6125 volts, those *with* the tongue gave ·6300, showing an increase of nearly two-hundredths of a volt. This increase is small, but it is decided, and shows that no power is lost by the tongue, but rather that a slight gain is obtained.* The internal resistance with the tongue was three-tenths of an ohm less.

Professor FORSTER: What was the whole resistance?

Mr. WRAY: ·2 ohms, as against ·23. If the tongue goes the whole length of the bar the electric force is diminished, but as we use it it is increased. I cannot explain the reason. I should have thought it would have short-circuited as you seem to infer. If the elongation were at the *other* end the current would be diminished.

Professor FORSTER: Is the tongue put in from the heated end or the cold end?

Mr. WRAY: It is from the cold end—the outer end.

I may remark that casting the bars under pressure *increases* the electromotive force. Here is the result of two bars cast from the same metal: one bar *without* pressure gave ·0843, and one *with* six inches head of pressure gave ·0885, showing an increase of

* Having reference to this observation the following note has since been supplied by Mr. Wray: "Probably from a defective bar, or other similar cause, the above results were as I stated; but by a series of tests (made in a manner so as to insure the greatest exactitude) we have since found that there is a *loss* of about 7 per cent. in electromotive force, and a *gain* of about 12 per cent. in resistance"—L. W. [ED.]

·0042 volt. We arrived at these results by means of Clark's Potentiometer.

The President having called upon Mr. Clark to reply upon the discussion,—

Mr. Latimer Clark said he had nothing to add to what he had stated. He was glad that this interesting discussion had been elicited, and he felt himself happy in having been the means of bringing the important subject of thermo-electric piles under the consideration of the Society.

On the motion of the President, a vote of thanks for the exhaustive and elaborate manner in which he had treated and illustrated the subject of his paper was accorded Mr. Clark by acclamation.

Mr. Jamieson was then called upon to read his paper "On a New Form of Lightning Protectors for Telegraph Wires and Apparatus."

ON A NEW FORM OF LIGHTNING-PROTECTOR FOR TELEGRAPH LINES AND APPARATUS.

By Mr. Andrew Jamieson.

Mr. President and Gentlemen,

With your kind permission I beg to bring before your notice a subject, the intrinsic value of which to the Telegraph Engineer will, I hope, excuse my boldness in intruding on the precincts of a paper so ably written and so well conducted as was that by Mr. Preece on "Lightning and Lightning-Conductors" in the year 1872.

In that paper the author pointed out that from the 1st of January to the 31st of July in that year 9·46 per cent. of the various instruments employed in the Postal Telegraph system in England were destroyed, either by the direct or indirect effects of lightning. Whether this large percentage has been decreased of late years by more care, so far as the means of defence are concerned, I am not in a position to say, though I hope this information will be elicited during the discussion upon this paper. Certain, however, it is,

that such an alarming statement demands more careful attention in future, and forms in itself an important argument as to the necessity for the employment of Lightning Protectors on telegraph circuits.

Strange as it may appear, the damage thus incurred is seldom due to the direct passage of lightning, but rather to what is metaphorically termed the return shock, and though many cases may be instanced in which the effects have been so severe, and the damage incurred so great, as to warrant the unavoidable conclusion that they must have been caused by lightning itself, yet these cases form but exceptions to the general rule.

The *rationale* of the agencies at work which produce the effects generally observed may be expressed as follows:—

On the formation of a thundercloud, the surface of the earth and all bodies directly beneath the cloud, or sufficiently near to be influenced by it, become inductively charged with electricity of the opposite sign, so that if a line of telegraph be subject to its influence a high-tension charge will accumulate upon the surface of the wire. If the inducing cloud be suddenly discharged, then this induced charge becomes liberated, and, in effecting its escape to earth at either one or both ends of the line, fuses the coils, and otherwise damages the instruments in circuit, unless special steps be taken to divert its course.

As to whether or not subterranean wires are subject to a like danger I am unable to say, never having had experience in them, but I should think that they would be, although, perhaps, to a less degree than land-lines. I may, however, here quote a letter which I received from Mr. Gerhardi in answer to some inquiries which I made on the subject.

"Dear Sir,

"Referring to your letter of the 4th instant.

"The case of the fine wire in a Walker's Lightning Preserver being fused during the prevalence of a severe thunderstorm at Bilbao occurred on 10th June last at the Bilbao landing of our Santander cable.

"There is no overground or uncovered wire whatever in circuit between the two stations of Bilbao and Santander, which were working together at the time of the occurrence, the line being composed of—

"1st. $8\frac{1}{2}$ miles of cable buried underground at an average depth of 18 inches;

"2nd. 46 knots of submarine cable from Las Arenas to Sardiñero;

"3rd. $1\frac{1}{3}$ mile of cable buried underground at a depth of 1 mètre from Sardiñero to Santander office.

"As is generally the case in these preservers, there was a great quantity of wire wound round the bobbin, of which only a small length, about $\frac{1}{2}$ an inch, I should think, was fused, and that in about the middle of the outside layer of wire. A black spot on the inner surface of the cylinder marked the place where the discharge had escaped to earth, but the fine wire did not, however, touch the cylinder, as the line was, immediately after the discharge, found to be insulated, the broken end of the wire having probably been fused. All communication was naturally interrupted till the preserver was taken out of circuit.

"An exactly similar case occurred during a thunderstorm at Santander last year upon the then Santander-Lizard cable, when there were in circuit the $1\frac{1}{3}$ mile of underground land-line mentioned, and 511 knots of submarine cable to the Lizard office.

"I am, dear Sir, yours faithfully,

"CH. GERHARDI, Manager."

The inducement which led me to endeavour to improve if possible upon the means of protection afforded by the apparatus usually employed for this purpose was found in the fact that the instruments on the lines of the Western and Brazilian Telegraph Company, upon which I was engaged, were peculiarly liable to damage from lightning. These lines were laid out as follows:—

A cable, the possession of the above Company, stretches from Rio de Janeiro to Montevideo, and is intersected at three points, namely, Santos, Santa Catharina, and Rio Grande-do-Sul. Its total length is 1,246 knots.

Starting from Rio Janeiro—one terminal of the entire line—the town-office is connected with the cable-house by a land-line, seven miles in length, which runs half-way through the town, along the side of a bay, over a mountain, and after skirting the sea-shore for some distance enters the cable-house, where it joins the cable through lightning-protectors.

From this point the cable proceeds to Santos, about 230 miles distant, and thence again to Santa Catharina, some 290 more. From the cable-house at the former station two land-lines, each 3½ miles in length, place it in connection with the town-office, where the instruments are placed in circuit, so that the land-lines form a loop with the Rio Janeiro—Santos and Santos—Santa Catharina sections. Exactly the same thing occurs at the remaining intermediate stations of Santa Catharina and Rio Grande-do-Sul, where pairs of land-lines, of 11½ and 13½ miles respectively, connect the towns with the cable-huts. At the former station the ground is exposed and very irregular, with long spans of wire at places where the nature of the locality compels their adoption, while at the latter the features of the country are of an entirely opposite character. The distance between the two towns is about 400 miles. From Rio Grande-do-Sul the cable continues on for 350 miles more to Montevideo, where it ends, and where a single land-line 1¼ mile long places the cable-house in connection with the town.

Several instances came to my notice of mirror and recorder coils being fused, especially at Santa Catharina, where they occurred as often as two or three times a month, although the instruments on these lines were always protected by the Siemens plate forms of lightning-protectors, but I never could discover that any of the cables had suffered damage, finding as they did an additional protection in Siemens's pointed form.

After ascertaining the fallibility of the ordinary plate protector I turned my attention to the general subject of lightning-protectors, with a view to devising, if possible, some form giving more certainty of protection to the instruments under my care. I carefully considered the mechanical and electrical details of a perfect lightning-protector, and compared all the instruments with which I was

acquainted with the standard which I thereby raised. In order to enable the members of this Society to judge how far I have accomplished my task I cannot do better than rehearse those principles upon which I laid the foundation of my comparison.

It will, I think, be unnecessary for me to enter to any great extent into the mechanical properties which a protector should possess, or the merits already possessed by the forms now in general use. For foreign service, at least, the instrument should be as light and compact as possible, not too expensive, and so made that should any portion become damaged it may be easily replaced.

The electrical qualifications of a good lightning-protector, although few, are of the greatest importance. They may, I think, be summed up as follows:—

1. Efficiency of action.

2. Its use should in no way interfere with the circuit arrangements.

3. In doing its duty it should not break down the circuit.

In order to prevent any infringement upon the second qualification it will be obvious that we must make use of some property possessed by high-tension electricity, distinct from those of electricity of the opposite nature. All the effects produced by electricity may be included in the following list, assuming light to be attendant on and inseparable from heat:—

1. Mechanical
2. Thermal.
3. Chemical.
4. Physiological.

The chemical properties of electricity are only met with, in practice, in connection with the battery, and those of the physiological class are never employed. We may therefore dismiss these from our consideration.

Electricity can produce mechanical and thermal effects, either directly or indirectly, through the medium of induction. Mechanical action in each case is either electrostatic or electromagnetic. To the former class belong all the attractions and repulsions brought about by frictional electricity, original or induced; to

the latter all movements of currents and magnets during the passage of dynamic electricity. Induction, however, plays such an important part in the operation of high-tension electricity, and so many forms of protectors are based upon its principles, that it will be advantageous to examine its phenomena before we proceed to the thermal effects of electricity.

Induction always takes place between two conducting surfaces separated by an insulator. Its amount varies directly as the area of the surfaces opposed to each other, provided they be parallel or concentric, also directly with the difference of potential between the two, and inversely as the thickness of the intervening insulator, or dielectric as it is termed. It therefore equals in mathematical language $\frac{a\,(v - v_1)}{t}\,k$, where a is the area of the plane surfaces, $v - v_1$ the difference of potential between the two and t the thickness of the dielectric, and k a constant differing with the substance of which the dielectric is formed. Taking air as unity, gutta-percha is 4·2 and india-rubber 2·8. When the induction between two such surfaces is very great, *i.e.* when the thickness of the dielectric is very small, the difference of potential between both its sides becomes so great that a *discharge* takes place. In the case of air, a spark or sparks pass, and the electricities on the surface of the conductors are neutralised and disappear. In the case of a solid dielectric its substance becomes pierced, and the neutralisation of the electricities takes place at that moment as before. Faraday supposed that the particles of the dielectric became polarised, and that, when the attraction of the electricities contained by the contiguous particles became greater than the attraction of cohesion, rupture ensued; irreparable in the case of solids, though, from their nature, liquids soon fill up the punctures.

We can now understand the *rationale* of the Siemens Plate Protector. That instrument is composed of two iron plates, separated by a thin film of air, the space between the plates being regulated by ebonite washers. The upper plate is connected to the line, and must therefore be at the same potential. The lower plate is connected directly to earth. The difference of potential between the line and earth when working is far too small to occasion a dis-

charge between the plates; but a static charge, like that which accumulates when the line is under the influence of the thunder-cloud, darts at once across the dielectric, instead of traversing the coils of the instruments.

The specific inductive capacity of air is found to greatly increase when its density is lessened, and a partial vacuum may be said to be a good conductor of statical electricity,—so much so, indeed, that at sea lightning has been known to divert its course from a thick copper lightning conductor in order to traverse a rarefied column of air behind the mast, caused by the rapid motion of the ship through the atmosphere. The most efficient protectors yet made are constructed on this principle, Varley's Vacuum Protector being among the number. They are, however, as a rule, expensive; and the existence of the vacuum is not always ascertainable, since by inspection it is impossible to say whether a vacuum is maintained or not. There can, however, be little doubt that, so far as efficiency is concerned, they are the nearest approach to perfection at which we have yet arrived.

Thermal effects are produced through the influence of induction, as already shown, or by the passage of a current through a wire. The thinner the wire, and the greater the strength of the current, the sooner we shall obtain a given amount of heat. This principle is advantageously made use of in the construction of many lightning-protectors—the ordinary working current not being of sufficient strength to fuse the fine wire of a bobbin, while the passage of a very strong current does so immediately.

A very important point, however, still remains, viz., the principle of points. It is well known that a point will give off electricity with far greater facility than a spherical conductor. The explanation which Professor Fleeming Jenkin gives for this peculiar phenomenon is, that the capacity of a spherical conductor varies *directly* as the square of the radius, whereas the density, *i.e.* quantity per unit area, varies *inversely* as the square of the radius, so that the less the radius of the sphere the greater will be the density. Consequently on a point, which we may assume to be an infinitely small sphere, the density will be infinitely great. It will in fact repel its own parts infinitely, and we can thus understand that a point

will give off electricity at a much lower potential than any other form.

If, then, we combine the point principle with that of the fine wire, or arrester, as it may with propriety be termed, we may when they are properly arranged expect to meet with very satisfactory results. The question with which we have now to deal is how best to carry out these results in order to obtain their best effect. We may perhaps here take a cursory view of their use up to the present time.

The very first lightning-protectors in use in England were formed of two points, opposed to and insulated from each other, to which the in-and-out wires were respectively connected. Later on several long points interlacing with each other, and which might more appropriately be termed spikes, were inclosed in a case, and so made that each point was opposed to one of the two plates, which respectively formed the line and earth connections. Later on the original principle of opposing point to point was again adopted, and Messrs. Siemens introduced at that time an ingenious device, which has remained to the present day almost unaltered. I allude to the grooving of the plates of their ordinary protector at right angles to each other. A number of grooves are made on the under and upper surface of each plate in such a manner that a number of comparatively sharp ridges are formed at right angles to each other. It will thus be seen that at every point at which any one of the ridges of the upper plate intersects, so to speak, those of the other, two points are formed, and it is a modification of this principle which I propose to use in the present instance.

So far as I am aware, the introduction of the *arrester* is of a more modern date, and it will therefore be unnecessary to trace the gradual fulfilment of the law of natural selection in its case. It is worthy of remark, however, that it is by no means necessary to use so great a length of wire as is generally employed in instruments of this nature. Three or four inches of copper or platinum wire, ·003 inch in diameter, is, I think, amply sufficient. In support of this fact I may add that no case has ever come to my notice of recorder coils, composed of about 300 yards of wire of the above gauge, being fused more than an inch from the terminal, and I never saw the wire of a galvanometer fused for more than two

inches and a-half, a fact which has since been confirmed by s experiments made by Mr. Munro. The letter from Mr. Gerha already quoted, gives a further corroboration of the truth of remark. The great drawback, as already mentioned, to the us the arrester is, that, as soon as it has performed its office, it br down the circuit; for this, however, I have made special pr sion, as will be seen from the description which I am now a to give, together with the accompanying diagrams. I must t leave it to your decision whether or not I have successfully formed the task which I set myself.

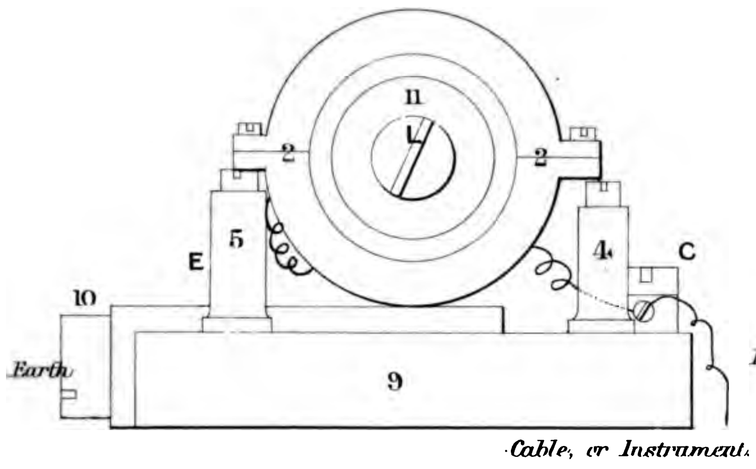
The accompanying diagrams show an outside plan, end elevat sectional plan, and cross-section of the instrument.

The several parts in each of these views are correspondin marked by letters and figures.

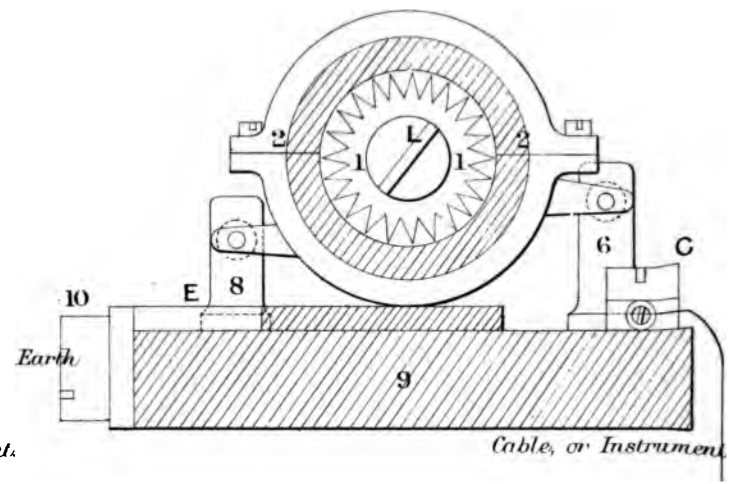
The land-line or underground wire is attached to the termi (**L**), and the cable condenser or instrument to (**C**); between th two points the ordinary battery current for effecting signals pa without leakage, but the lightning, or its induced current, is trapp arrested, or conducted to earth.

(**L**) is one terminal of the inside corrugated, or toothed, cylin (1), which cylinder is supported centrically, inside an outer cylin (2), by ebonite washers (11) and (12). These washers are fi into recesses in the ends of (2), and so arranged that cylinder is separated and insulated from cylinder (2) by as fine an interveni air space, or vacuum, as can be mechanically adjusted. Cylin (2) is bored and tapped throughout its length by a fine poin deep screw of from 40 to any desired number of threads. It w thus be clearly seen that if we have (say) 30 longitudinal corru tions or fine pointed teeth cut on cylinder (1), and 50 threads cylinder (2), we shall have 1,500 crossings, or opposing poin presented for facilitating the discharge of electricity of high tensi from cylinder (1) to (2), the latter being connected to earth by t brass slab (**E**), upon which it rests, and a flat copper band leadin from terminal (10) screwed thereon. (3) is the other terminal o cylinder (1), to which is fixed a fine steel spring. Between th end of this spring and terminal (4) is tightly stretched a fin platinum or copper wire ·003 inch in diameter. Terminal (4)

END VIEW.



CROSS SECTION OF CYLINDER 2.
THROUGH LINE A. B.



ARRESTER

connected to terminal (5) by a flat copper strap, running underneath the ebonite slab, upon which the whole instrument is supported. Between terminal (5) and (**C**) is run a silk-covered platinum wire ·008 inch in diameter (previously steeped in paraffine or shellac), with several turns taken round the outside cylinder (2), or, if preferred, any other earth connection.

In order that the fusing or breaking of the fine wire stretched between terminal (4) and the spring attached to terminal (3) may not stop telegraphic communication a terminal (6) is so placed and provided with an adjustable platinum pointed screw as to make contact with the spring upon its being released. A similar connection is formed between (6) (7) and terminal (5) by an underneath copper strap spring and a second fine wire. Should this wire be fused or broken the spring attached to terminal (7) will be released and make connection with a platinum-pointed screw in terminal (8), which is in good metallic connection with the brass-plate (9), and therefore with earth, thus effectually earthing the end of the land-line or underground wire and preventing a possibility of mistake as to where the loss of continuity has taken place.

The ordinary message current passes from land-line or underground wire at (**L**) along cylinder (1) to terminal (3) by spring and fine wire to (4), by underneath connection to (5), and platinum wire wound round cylinder (2) or earth connection to (**C**), and thence to cable, condenser, or instrument. Should the fine wire between (4) and spring (3) be broken, it goes from (3) to (6) by underneath connection to (7), spring and fine wire to (5), and thence as before to (**C**), cable, condenser, or instrument.

If lightning or a high tension electrical current pass along the land-line or underground wire it will, so far, if not altogether, be trapped to earth through the fine film of air (or vacuum if such has been provided for) between the points of cylinders (1) and (2). Should any part of this current pass beyond terminal (3) it is offered the chance of fusing the fine wire stretched between the end of spring (3) and terminal (4) (or spring at 7 and terminal 5).

Further, in order to lessen the disturbing and damaging effects upon the cable, condenser, or instrument, by any portion of the high-tension current which might pass through the stretched wires

before fusion takes place, I have arranged the larger wire, between (5) and (C), covered with silk and an insulating substance, so as to bring into action the principle of conductive discharge between this wire and the earthed cylinder (2).

The whole instrument is fitted to an ebonite base (9) and inserted in a neat case or box, with glass top and key, having convenient holes for the leading-in wires. Cylinder 2 is made in halves so as to facilitate its being easily examined in case of a strong charge fusing its interior points to that of cylinder (1).

There is nothing new to be claimed in the several features of this protector and arrester as viewed from an electrical point of view, but I think I am correct in saying that they have never before been all combined in one instrument and in a similar manner.

I may here state that the above plan of attaining opposite points by having longitudinal teeth in the one conductor and a screw thread in the other was designed by me while out in Brazil for the Western and Brazilian Telegraph Company, in February 1875, although I have found since writing this paper that a very similar design had been previously carried out by Siemens as well as by a French gentleman named Lemasson.

Since this form of protector and arrester has been applied to the Western and Brazilian Telegraph Company's lines and apparatus there has not been a single case of injury to their cables or instruments by lightning, so far as I can learn.

The PRESIDENT: We have another,—a short communication on lightning protectors, which the Secretary will now read. Walker's conductors having been named, I may call attention to specimens on the table before you. Among them there are two conductors which have been called upon to do duty. The points, as you will see, are fused, and the fine wires burnt. The length of fine wire is less than the author of the paper thought. There are only three layers on a wooden bobbin. The points are on both sides of the coil of fine wire, and are presented to a brass cylinder connected with the earth, within which cylinder is the bobbin of fine wire.

The SECRETARY read a communication from Mr. Webber, of the South Devon and Cornwall Railway; explaining two forms of lightning protectors in use on that Company's system.

The first of these protectors is composed of two semi-circular pieces of brass, about an inch in diameter, arranged with their diameters opposite each other, set close together, yet sufficiently apart to prevent the passage of a working current. To them are attached the *in* and *out* wires of the coil or instrument to be protected. These semi-circular pieces are fixed to a piece of wood, and the whole inclosed by a flat brass cylinder, to which is attached a terminal, for the purpose of connecting it with the earth, the space between the sides of the cylinder and semi-circular pieces being regulated similar to that between the latter, and the space between each being filled in with gypsum, reduced by mixing with water to a thick paste, and afterwards allowed to dry. After drying for a day or two the gypsum becomes a hard crust, which, although a bad conductor of low-tension electricity, yet facilitates, in some degree, the passage of lightning.

The second protector differs only in construction from the one just described. To a wooden base about three inches by two are screwed three thin brass plates, each about half-an-inch in breadth. The edges of these plates, which oppose each other as they lie side by side, are sharpened and pointed, and the plates are then set as near to each other as they can be without touching. To each of the side plates are fixed two instrument terminals, while to the centre plate one only is fixed. A glutinous mixture of powdered carbon and gum-water is employed to fill up the space between these plates, which when allowed to harden serves the same purpose as does the gypsum in the instrument already described. As before, the wires coming from the instrument, or that part of the apparatus to be protected from the effects of lightning, are connected to the side plates, one to each respectively, and the line and earth wires are connected in a similar manner, while the centre plate is put to earth.

The discussion was adjourned till the next Meeting.

The Fifty-first Ordinary and Fifth Annual General Meeting was held on Wednesday, the 13th December, 1876, Mr. C. V. WALKER, F.R.S., President, in the Chair.

The PRESIDENT said: I have the pleasure to announce that a very beautiful marble bust of the late Sir Francis Ronald has been presented to this Society by Dr. Siemens; also that a portrait of the late Mr. J. L. Ricardo, whose name is so well known from his intimate connection with the early progress of the electric telegraph, has been presented by Mr. Frank Webb; and also a portrait of the late Alexander Bain, presented by Mr. Latimer Clark. The latter gentleman has also presented to the Society a collection of curious specimens of the telegraph apparatus of early days; and I have myself had the opportunity of presenting a small contribution to the early history of telegraphy, viz.: the original gutta-percha joint-tongs, contrived by Mr. Thomas Forster, of Streatham, in 1848. These contributions will be exhibited at the *soirée* to be held on Monday next.

Scrutineers were appointed for the ballot for President and Council for the ensuing year; and the President announced that the ballot would be opened and remain so until half-past eight o'clock.

The Acting Secretary then read the Annual Report of the Council, as follows:—

ANNUAL REPORT FOR 1876.

The time has again arrived when it becomes the duty of your President and Council to render an account of their stewardship and announce to the Members the progress of the Society during the past year as well as its present condition.

The President and Council of the Institution of Civil Engineers have extended the same privilege and behaved in the same handsome manner towards the Society during the past year as they have done in every previous year since its foundation. Our best thanks are due to them, for to their fostering help the continued success of the Society is in a great measure to be attributed.

The Council have resolved to increase the number of Local Honorary Secretaries, and steps are now being taken for the appointment of gentlemen to act in that capacity in those countries where the Society is not at present represented. That such a course must prove to be of the highest advantage is evidenced by the list of new candidates from Paris announced this evening, all of whom have put their names forward owing more or less to the representation of Mr. John Aylmer, whose appointment as Honorary Secretary for France was announced at the last Annual General Meeting.

During the past year Mr. E. C. Cracknell, Superintendent of Telegraphs in New South Wales, has been appointed Hon. Sec. for Australia, and Mr. G. G. Ward, Superintendent in New York of the Direct United States Cable Company, has received the same appointment for the United States. The list of Honorary Secretaries at the present moment therefore is as follows :—

JOHN AYLMER, Civil Engineer, 4, Rue de Naples, Paris,	FRANCE.
W. E. AYRTON, Professor of Natural Philosophy, Imperial College, Tokei, Japan,	JAPAN.

CHARLES BURTON, Telegraph Engineer, Buenos Ayres,	ARGENTINE REPUBLIC.
E. C. CRACKNELL, Superintendent of Telegraphs for the Colony of New South Wales, Sydney,	AUSTRALIA.
Le Commandeur E. D'AMICO, Director-General of the Italian Telegraphs, Rome,	ITALY.
FRÉDÉRIC DELARGE, Engineer of the Belgian Telegraphs, Brussels,	BELGIUM.
C. L. MADSEN, Great Northern Telegraph Company, Copenhagen,	DENMARK.
C. NIELSEN, Director-General of the Norwegian Telegraphs, Christiania,	NORWAY.
Don RAMON VIAL, Director-General of the Chilian Telegraphs, Santiago,	CHILI.
G. G. WARD, General Superintendent of the Direct United States Telegraph Company, New York,	UNITED STATES OF NORTH AMERICA.

The additions to the ranks of the Society during the past ye number in all 76. These comprise—

18 Foreign Members,
11 Members,
45 Associates,
2 Students.

Thus the Society now numbers 751 names, of which—

5 are Hon. Members,
125 Foreign Members,
226 Members,
380 Associates,
15 Students.

The list of Candidates to be balloted for to-night (21 in all), as well as an equal number of candidates for admission, whose names have just been announced, show that the advantages of the Society do not fail to be appreciated, and that its sphere of usefulness is extending as satisfactorily as can be hoped for.

The name of Mr. Scudamore, in recognition of the valuable services rendered by him to Telegraphic extension, has been added to the list of Honorary Members.

The Council would draw attention to the number of Associates, many of whose names are so well known in Telegraphy that their election to membership would, it is believed, be an honour alike to themselves and the Society. The grade of Associate was established at the outset for those whose "pursuits constitute branches of Electrical Engineering or who are so intimately associated with the science of Electricity or the progress of Telegraphy that the Council consider their admission as Associates would conduce to the interests of the Society." Many who joined as Associates have now won a position, or otherwise made a mark for themselves, which entitles them to admission into the higher grade of membership, and the Council therefore invite applications from the Associates with a view to their transference.

The Council regret to have to announce that but slow progress has been made with the catalogue of the Ronald's Library. This has been unavoidable for various reasons. Amongst others, the fact that the publication of the Proceedings of the Society had fallen considerably in arrears and had to be worked up. These are now printed up to date, and the Journal containing an account of the proceedings of the Society up to the close of last Session is completed, and will be issued to the Members generally in the course of the next few days. Now, it is hoped that the promise made some time since of the Journal being issued quarterly will

STATEMENT OF RECEIPTS

By the Treasurer, for the

RECEIPTS.

		£ s. d.	£ s. d.
By Balance 31st Decr. 1875	... ...		162 11 1
„ Subscriptions	1872	4 3 0	
„ „	1873	24 16 0	
„ „	1874	69 10 6	
„ „	1875	252 17 2	
„ „	1876	613 0 5	
„ „	1877	16 4 0	
„ „	1878	1 0 0	
			981 11 1
„ Publishing Fund	... ...		8 10 6
„ Sale of Journals	... ...		49 12 4
„ Composition of Life Subscriptions	... ...		30 0 0
„ Donation to Library	... ...		5 0 0
„ Miscellaneous	... ...		0 17 8
		Total	£1,238 2 8

STATEMENT OF

On 31st

RECEIPTS.

	£ s. d.
By Donations	200 0 0
„ Compositions	191 15 0
Total	£391 15 0

ESTIMATE OF ASSETS

On the 31st

LIABILITIES.

		£ s d.
By Subscriptions in advance	... ...	20 4 0
„ Outstanding Accounts—Salaries	... ...	4 19 0
	Short-hand reporting	3 3 0
	Attendance and Refreshment	1 14 10
	Rent, Fuel, Taxes, and Cleaning	60 0 0
	Printing and Stationery	659 19 0
		749 19 10
	Balance Cr. ...	871 14 11
		£1,621 14 9

AND EXPENDITURE

year ending 31*st December*, 1876.

EXPENDITURE.

	£	s.	d.	£	s.	d.
To Salaries—Secretary and Clerical Assistance ...	202	9	8			
Clerical Assistance to Treasurer	15	0	0			
				217	9	8
„ Shorthand Reporter				17	6	6
„ Attendance and Refreshments at Meetings				17	10	0
„ Printing and Stationery				432	19	1
„ Furniture and Fittings				90	7	3
„ Rent, Taxes, Fuel, and Cleaning				242	3	6
„ Overpaid Subscriptions refunded				11	16	0
„ Sir F. Ronald's Library—Insurance	5	0	0			
Legal Expenses	9	11	11			
				14	11	11
„ Petty Expenses, including Postage and cost of issue of Journals—						
Secretary	56	15	0			
Treasurer	7	17	2			
				64	12	2
Balance Cr.				129	6	7
Total				£1,238	2	8

(Signed) C. E. WEBBER, MAJOR R.E., *Hon. Treasurer.*
J. SIVEWRIGHT, *Acting Secretary.*

We have compared the above Account with the Vouchers and Cash Books, and find it to be correct, leaving in the hands of the Treasurer one hundred and twenty-nine pounds, six shillings, and seven pence.

J. WAGSTAFF BLUNDELL, }
FRED. CHAS. DANVERS, } *Auditors.*

CAPITAL ACCOUNT

December, 1876.

EXPENDITURE.

	£	s.	d.
To Furniture and Fittings	376	4	6
Balance Cr.	15	10	6
Total ...	£391	15	0

AND LIABILITIES

December, 1876.

ASSETS.

	£	s.	d.
To Unpaid Subscriptions	650	0	0
„ Furniture	210	0	0
„ Journals sold or in hand	303	13	0
„ „ in hand of publishers	113	15	2
„ Objects presented to Society	215	0	0
Cash in hand	129	6	7
	£1,621	14	9

be fulfilled, but the Council would at the same time take this opportunity of again asking the Members of the Society to contribute towards this end. Original communications, more than anything else, are wanted, and with a properly-conducted Journal no better medium it is hoped now exists for the publication of researches in Electricity or inventions in Telegraphy than through the Journal of the Society.

A circular has been addressed during the past year to the Members on the subject of Papers to be read, and it is gratifying to be able to announce that communications, in all cases of value, have been already forwarded or voluntarily promised, which will entirely occupy the evenings at our disposal before the close of the Session.

There is but one point more on which the Council feel it their duty to dwell, and that is the payment of arrears of subscriptions. The list of these, although considerably reduced, is still a great deal larger than it ought to be, and some special steps will have to be taken for recovering them, or for the removal from the Society's books of those who, notwithstanding every appeal, still decline to subscribe what is owing by them. But for this the Council would have had the utmost pleasure in congratulating the Society upon the prospect immediately before them of a Session of considerable promise.

On the motion of Mr. Gerhardi, seconded by Mr. Langdon, the Report was unanimously approved and adopted.

The PRESIDENT: Before opening the discussion on the paper read at the last meeting the Acting Secretary will read a communication which has reached us from Mr. Aylmer, our Honorary Secretary in Paris, "On a form of Lightning Conductor used on the State lines of France."

The Acting Secretary then read the following communication:—

Paris, 9th December, 1876.

DEAR SIR,—As I see that the subject of Lightning Guards is to be again before the Society at their next meeting, I thought it might interest the members to hear something about the principal

form of guard used by the French Administration on the State lines. I have to-day sent you one of each of the two patterns in use for the members to see, and shall feel obliged if you will read them the short description I inclose herewith. I may mention that, with the exception of the fine platinum wire, an exactly similar guard to that designed by Mr. Andrew Jamieson has been under trial in this country for some time back.

I am, dear Sir,

Yours very faithfully,

J. AYLMER.

The Secretary of the
SOCIETY OF TELEGRAPH ENGINEERS,
London.

BERTSCH'S LIGHTNING GUARDS.

The principle upon which these lightning guards are constructed is that of a combination of plates with a considerable number of very fine points. They are made of two forms, one, which is inclosed in a strong and water-tight cast-iron box, is intended for fixing outside a station, upon the terminal pole of a line, at the mouth of a tunnel in which there are covered wires to protect, or at the junction of overhead with underground wires; the other, which is preserved from dust and injury by a light wooden box, is placed inside the station as near as convenient to the entry of the line wire.

The first of these guards is composed of two brass plates, each carrying 300 fine silvered points. These plates are insulated from each other, and are placed so that the points of one plate are exactly opposite those of the other, and at a uniform distance of about one millimeter two binding screws fixed outside the cast-iron box, but insulated from it by blocks of ebonite, are in connection with one plate, the other being in electric communication with the metal box. The line wires are attached to the binding screws on the front of the box, and the earth wire is held under the head of one of the four coach screws which fix the apparatus to the wall or

pole. The iron box has strongly glazed sides to permit the state of the inside being seen.

The second guard consists of three brass plates; the uppermost or first of them carries 300 fine silvered points, which are held at a distance of one millimeter from the second or middle plate. This latter one rests upon a third or last plate, but is insulated from it by the interposition of a piece of varnished sheet gutta-percha or paraffined paper. Two metal pillars screwed into the bottom plate hold the top one in its place and make electric communication between the two. The line wires are attached to binding screws carried by the middle plate, and the earth wire to a screw on the third or lower plate. This guard acts as a double discharger, for an atmospheric charge entering the centre plate is, if weak, drawn off by the points, or, if strong, goes direct to earth by traversing the gutta-percha or paper sheet separating the centre from the under plate. After this latter happens it may be necessary to replace the insulating sheet with a fresh one.

Several thousands of both these lightning guards have been brought into service by the French Telegraph Admininstration at different times during the last eight years. They continue to give every satisfaction, and are still largely ordered.

The French Railway Companies also use them, especially for the protection of covered wires in the tunnels.

Mr. ANDREW JAMIESON then read some supplemental remarks to his paper, and gave a further description of his lightning protector.

The PRESIDENT: Before calling upon the Members for their observations on this subject, I would revert to the last meeting, when "Walker's Lightning Protector" was mentioned. As it was of very ancient date I was unable to recall its history at the moment. I have brought one or two of them here to-night. This [exhibiting] is one of the conductors complete, ready to place in the circuit of a telegraph wire. Its parts are a small wooden bobbin, about an inch in length, and a stout brass cylinder for earth-connection outside the bobbin, $1\frac{1}{2}$ inch long and five-eighths inside diameter. The total length of the little instrument is $3\frac{1}{2}$ inches, including a terminal at each end. Nine yards, weighing 21 grains, of No. 40 copper wire are wound in three layers on the bobbin, and

complete the circuit between terminal and terminal. The foot of one of the terminals presents three fine points to the earth-cylinder. The earth-cylinder presents three like points to the foot of the other terminal. The former terminal is screwed into the bobbin in connection with one end of the fine wire; it carries a spur, which presents six fine points to the earth-cylinder. Here [exhibiting] is one which has suffered in protecting an instrument from lightning, and can be examined. The points of the spur are fused, as is also some of the solid brass on the inner side of the cylinder. Here [exhibiting] is another, which has protected an instrument, and the spur points are entirely fused away. Here [exhibiting] is also one which has done duty; the inside of the cylinder is much fused. These instruments were brought into use in the year 1848. Referring back to my diary, I find these entries:—

1848, July 15. Tunbridge Wells.—Coils of Tunbridge instrument and bell fused. New lightning conductors not up.

1848, September 25. Margate.—Fitted my lightning conductors.

1848, October 6. London.—Fitted with movable and lightning conductors.

1848, October 28. Short storm. Tunbridge Wells.—Lightning entered; darted from bobbing wire of lightning conductor to cylinder; burnt off silk; connected wire with cylinder, and saved the instrument.

Of late years I have attributed a great deal of the immunity of our telegraph lines from serious damage by lightning to the fact of the block system being universally adopted, so that the wires generally terminate in earth so frequently. There are lying on the table two of the oldest lightning protectors with which I am acquainted. That with balls and points was used before 1845. The North Kent line, erected by Mr. (now Sir W. F.) Cooke in 1849, was provided with a double-barrelled lightning conductor, somewhat the same in principle as mine. The instrument you will find fully described in the *Electric Telegraph Manipulator*, published in 1851. I shall now be glad to hear any remarks which gentlemen may have to offer on the subject.

Mr. LATIMER CLARK: I beg to bring under the notice of the Society a principle of construction of Lightning Protectors which I invented a long time ago, and which I think was worthy of more notice than it received at the time. It is well known that when two plates are laid near together, owing to the effect of induction the facility with which a spark passes between the two plates is not so great as when a point is presented to a plate. But with lightning protectors having points it depends upon mechanical skill how near you can get these points without danger of contact. My object was to enable you to insure close proximity of the points without reference to the skill of the mechanic. I have a model here [exhibiting]. This bottom plate is an earth-plate of iron. Upon this I place either a thick film of varnish or a piece of thin carbon paper, on which I place the upper plate, pierced full of holes, each of which contains a loose pin or point. The peculiarity is that these points are all free to slide vertically within the plate so loose that they all fall down and rest on their points on the paper covering of the plate, so that when put into position each point is separated from the earth-plate only by the thickness of the varnish or tissue on which it rests. The whole are therefore at a uniform distance from the earth-plates dependent upon the thickness of the film. I think upon that principle a useful form of lightning protector may be constructed, although I have not myself attempted to bring it into practical use. I would merely add, in cases like those Mr. Jamieson described at the last meeting, where the protector has to be placed at some distance from the office, it would, I think, be an advantage to adopt some arrangement by which, through the agency of an electro-magnet, by sending a current the upper plate may be lifted up from the lower one; the act of lifting up the points and allowing them to fall again would break any contact or fusion that might have been caused by lightning, and might be useful also when testing the line in cases where the protector is in a cable house at some distance from the office. I may add, that a cheap form of this protector can be had by looping or suspending loose wire to the upper or line-plate, and allowing it to rest at an angle on the bottom plates, with tissue interposed; the mere shaking of the protector would at once break any connection formed by lightning between the line and earth.

Mr. R. von Fischer Treuenfeld: Mr. Jamieson's very neat construction of a lightning discharger is based on the principle of double security, and there can be no doubt that this method is safer than that of single security. Mr. Jamieson combines the Siemens plate system, which he terms "protector," with the fuzing of a fine platinum wire, called "arrester." The same desire for additional security in the construction of lightning dischargers led long ago to constructions of a very similar kind, and there is on the table an apparatus, exhibited by Messrs. Siemens, based on exactly the same two principles.

This lightning discharger consists of a massive brass cylinder with a fine spiral groove around its circumference; this cylinder is connected to earth. In the groove is laid a spiral of fine silk-covered platinum wire, which forms part of the line circuit. In this discharger the silk intervening between the platinum line circuit and the brass earth-cylinder represents the system of the plate lightning discharger, and the platinum wire itself the principle of the arrester.

With regard to the construction of lightning dischargers for ordinary land-line stations I beg to point out an important principle which appears to have been overlooked in some of the apparatus exhibited.

The principle I refer to is, that it is essential that a station lightning discharger be easily accessible, and it should admit of all the working parts being readily inspected by the station clerk without the necessity of unscrewing the apparatus or using any tools for the purpose of inspection. Such constructions as cannot be easily inspected so that the clerk may at once convince himself that the lightning discharger is not the source of the fault under observation, although based theoretically on better principles, must lead to the introduction of disadvantages and uncertainty, therefore I maintain the opinion that the old Siemens plate lightning discharger is still decidedly the most efficient for land lines of telegraph.

Mr. W. H. Preece: It is rather a strange assertion to make, but I have in my own mind no doubt whatever that we in England suffer more from atmospheric electricity than any other country on

the globe. It is not that thunderstorms are more frequent here, for we know that in this temperate climate they are few and far between. Nor are they more intense; because compared with the roaring lions that we hear of in tropical regions our thunderstorms are mere sucking doves. But it is because in this small country of ours we have an enormous network of nearly 170,000 miles of wire and about 20,000 instruments concentrated over a small area. Hence no little puny thunderstorm can approach our shores and spend itself upon our hills and valleys without embracing in its influence many miles of these wires and many of these instruments. Thus it is that the number of accidents from lightning in England are so numerous.

When I brought this subject before the Society on a previous occasion, I showed that from the 1st of January, 1872, to the end of July in the same year, there were no fewer than 897 lightning accidents. Taking the whole of 1872 together, there is no doubt that in England we experience at least 1,200 faults from lightning itself. No doubt the year 1872 was a year of maximum intensity. Probably thunderstorms, like auroræ and earth-currents, occur most at stated periods. The period of the two last is known to be about eleven years. Those periods have not yet been determined for thunderstorms. 1872 was, as I have said, a maximum season; since then the intensity of the storms has been gradually diminishing. During the past year, from Nov. 1st, 1875, to 30th Nov. 1876, the 1,200 lightning faults which were experienced in 1872 have fallen down to 442, so that in the last year, on the whole postal telegraph system of England, we have suffered 442 times from the effects of lightning.

Now lightning develops itself in two forms. There are the direct discharge and the induced current due to the approach, discharge, or departure of a charged cloud. We do not suffer much from the direct discharge. When the wires or apparatus form a path for the discharge from the cloud to the earth, everything of course is shattered. I have several records of such cases; some of them are interesting, and I will mention one which I have taken from a report from South Wales. On May 25th, 1876, a severe thunderstorm occurred about 8 a.m. in the neighbourhood of Tondu, last-

ing about an hour. One discharge destroyed a needle instrument, melted the iron line-wire, fused the gaspipe, lit the gas, and burnt part of the station. A horse, a cow, and thirty turkeys, were killed by the same flash.

The induced effect of charged clouds develops itself in the form of intense currents traversing our wires and operating our instruments. They vary in direction and strength. At times they cannot be distinguished from working currents. A postmaster observed, on August 24th, that a pale blue flash moved his A B C instrument one letter, a darker flash moved the indicator two letters, in three instances three letters, and another four, and in one instance the indicator was moved as many as ten letters. Thus, between the direct discharge and the working current we have various stages in the strength of the current produced by atmospheric electricity, producing various kinds of accidents on poles, wires, and apparatus. The poles have rarely suffered. Insulators were sometimes cracked and sometimes smashed. There was one authenticated instance of two insulators falling coincident with a flash, but no trace of lightning could be found upon them. I have seen insulators which gave distinct indication of carbonisation and discolourisation from the passage of the discharge. The leading-in wires frequently suffer, being frequently destroyed by the passage of the discharge. There was an instance of this at Appledore, North Devon, in which a terminal pole was thirty yards from the office. The discharge entered the line wire, and passed through the gutta-percha wire to the office. Twenty yards of this wire were entirely destroyed, and in many places the copper wire inside the gutta-percha was entirely dissipated, so that no trace whatever could be found for many inches of the presence of copper. The charge passed from the leading-in wire to the earth wire, and thence to a water-pipe under the flags. At the junction of the earth-wire and water-pipe the lead was fused and the water escaped. The casing was split to pieces for a short portion between the leading-in wire and the waterpipe. The earth-wire of No. 8 iron carried the charge away perfectly. These [exhibiting] are specimens of the way in which the leading-in wire has been twisted and melted, and in some places dissipated, and they are worthy the inspection of the Members of the Society.

Sometimes we have had accidents in underground wires. Two or three faults are shown on this board, where the charge made a complete hole through the gutta-percha, the edges being scarred, burnt, and carbonised; but it is in our apparatus we principally suffer, and of those 442 faults I have mentioned nearly all happened in the apparatus. The character of the faults is as follows:—198 coils were fused, and 75 magnets were demagnetised; 20 A B C instruments were deadjusted; 149 K protectors were fused or otherwise injured.

With regard to the mode of protection, Mr. Walker has brought before us an interesting historical record of what was done in this way in 1848-49. But the Electric Telegraph Company, who first established the commercial system of telegraphy of this country, speedily abandoned the use of protectors, for the simple reason that they found by experience that the protectors themselves produced more faults than they were designed to prevent. In those days wires were few, and instruments were scattered, and storms passed over the country harmlessly; but when the system was more extended and it fell into the hands of the Post Office it was evident that protectors became essential. Previous to that, however, Mr. C. F. Varley had taken advantage of the experiment of Faraday, which showed that electricity of high tension flew across a short air space in preference to taking a wire circuit. He simply knotted or twisted the two silk-covered ends of the coil-wire together without interfering with the working currents, but enabling currents of great intensity to fly across and to avoid the coil.

This was a protector as far as the evil was concerned, and it possessed the principle which Mr. Treuenfeld advocated, and with which I fully concur, that the protector should be capable of being easily repaired. The twisted wire was subsequently wound around a boxwood bobbin, which in its turn was replaced by a brass bobbin connected to earth, and that in its turn by a brass tube, around which a fine wire was coiled, so that by a process of evolution the protector used by the Post Office became this little protector which Mr. Treuenfeld has shown this evening.

Points have not been very much used in any form, but plate conductors are much used, and certainly show themselves to be

extremely efficient. Of the different plates there are two methods in use—one where the two plates are separated by an air space pure and simple, and the other in which the plates are separated by a piece of thin paraffin paper. It is yet a question to be decided which of these two methods is best. The air space fulfils the condition of Faraday's experiments. The paraffin paper places between the two plates a dielectric of very high resistance, and unless the paraffin paper has holes or defects in it it can be as effective as the air space. On the other hand it enables the plates to be brought very near each other, for ebonite washers between the two plates where an air space is used necessitates their being placed a greater distance apart than when separated by paraffin papers. It would appear that a compromise between the two methods is perhaps best —that is, two plates separated by perforated paraffin paper so managed that there shall always be an air space.

Perhaps the most efficient lightning protector is the vacuum tube of Mr. Varley, but it is not much adopted in practice, because it is so expensive. At all cable ends, however, and at many of the connections between open wires and long underground lines, wherever expense is no object, we place this vacuum protector, and wherever it has been used it has been very effectual. The difficulty is to maintain the vacuum, which can only be insured by constant inspection. They are periodically tested by passing sparks from small induction coils, which gives unmistakeable intimation of the pressure or absence of vacuum by the form and colour of the spark. Fine platinum wire is also used in connection with the cable. At all cable ends every precaution is taken to obtain great safety and security. The land-line is first attached to a plate protector, serrated according to Siemens' method; it is then connected by a piece of thin platinum wire, which is spirally arranged round an earth tube. From that it passes to the vacuum conductor, and thence to the cable; so that a charge passing from open wires to a cable has to overcome these three enemies—first, the plate discharger; next, the fine platinum wire which is coiled for a long length round a tube or barrel of brass connected with earth, all of which give every opportunity to dispose of the charge; and lastly, it has the vacuum to overcome: so that it stands to reason every

charge, however intense on reaching the land-line, is so reduced in potential before it enters the cable that it becomes perfectly harmless. The fine wire is abroad often placed next to the cable, but this is a fatal error, for the charge that fuses the wire must necessarily also enter the cable. If, however, the vacuum protector, or any protector, be between the fine wire and the cable, it may be fused without danger to the cable, for the protector will reduce the potential of the charge before it enters the cable.

In some cases lightning protectors are attached directly to the line-wires, the binding wires being turned down and the ends pointed in the direction of the pin of the insulator. This has proved to be a very effective lightning protector. Probably the most effective protectors in England are the earth-wires, which exist at nearly every pole. Notwithstanding the fact that nearly every pole in England has a lightning protector—every instrument has a protector of some sort or other—nevertheless in the past year we have suffered no less than 442 times from the effects of lightning. I do not mean to assert that lightning has defeated us; on the contrary, I think we have succeeded in defeating the lightning; but in point of fact there are currents between what are actual working currents and what are currents of sufficent intensity to make protectors operate, of such strength and duration as to fuse coils and deadjust apparatus. Hence we cannot hope to be entirely free from lightning interference. We have really made our lightning protectors too sensitive. They are too frequently subjected to currents of intensity not high enough to overcome air spaces, or spaces of paraffin paper, but strong enough to damage coils. However sensitive or however coarse we make the protectors we shall never thoroughly prevent lightning from injuring our apparatus. I do not think, however, we have yet obtained the desideratum of a thoroughly good protector. What we want is a safe protector—safe to protect our apparatus and safe to prevent putting on faults; and until we do possess such a protector I do not think it can be said that in this particular branch of our profession we have reached perfection.

Mr. R. von Fischer Treuenfeld: I am very much pleased to hear that the opinion of so high an authority as Mr. Preece, based

on the results obtained in the British Post Office Telegraphs, agrees with mine, that is, that the Siemens' lightning discharger is most efficient in its working.

Mr. Preece considers it an open question whether a plate discharger with a layer of paraffined paper between the plates is preferable to one with an air space only; the former he considers to have the disadvantage of introducing a high resistance to the atmospheric electricity; the latter is open to objection on account of the ebonite washers wearing away in the course of time and thereby changing the intervening air space.

I beg to draw Mr. Preece's attention to Siemens' plate lightning dischargers of more modern construction, which are entirely free from the objection of the ebonite washers. From the one exhibited on the table you will see that the line and earth-plates are maintained at their original distance from one another, with ebonite washers, and are not liable to any change of their relative positions.

Mr. Preece: My observations amounted to this: my objection to air spaces was that you could not bring your two plates so close together as you can when they are separated only by a thin piece of paraffin paper; you can then bring them to almost an infinitesimal distance from each other; and that, when you use an air space, you are liable to have the plates forced together by a discharge which would pass by harmlessly if paraffined paper be used. Personally, however, I prefer an air space to the interposition of any dielectric or conducting medium of high resistance, for an air space appears to me to fulfil exactly the conditions required for a lightning protector, that is, it offers an infinite resistance to the working currents and a very low resistance to currents of high intensity.

Mr. Saunders: As to the protection from lightning of cables connected with aerial lines I have always found that the fine wire-guard is sufficient.

The cables of the Mediterranean Extension Company from Sicily to Malta, and Otranto to Corfu, the former with about seventy, and the latter with about twelve miles of aerial line in circuit, were formerly protected by a fine wire of about three feet long, inclosed in a glass tube for safety. These wires were frequently fused and

the glass tube fused and broken, but in no case has there been any damage to the cables. Seeing that the glass tubes broke, gave me the idea of inclosing a spiral of fine wire in a brass tube (connected to earth), with discharging points at each end. By placing it horizontally when the wire was fused, the ends fell on to the inside of the tube and thus put both aerial line and cable to earth.

At Otranto the Eastern Company's Cable to Zante is also connected to a short length of aerial line. The wires were so often fused there that the superintendent, Mr. Eggington, designed a form of guard, exhibited on the table, combining the wire and plate guards.

I was in hopes of receiving from Malta some information as to the action of my tube lightning guard in time for this meeting, but it has not yet reached me.

The discussion was then closed, a vote of thanks, on the motion of the President, being unanimously accorded Mr. Jamieson for his paper.

The President announced the result of the ballot to be as follows:—

President.

PROFESSOR ABEL, F.R.S.

Vice-Presidents.

MAJOR J. U. BATEMAN-CHAMPAIN, R.E.
PROFESSOR G. C. FOSTER, F.R.S.
W. H. PREECE, M.I.C.E.
CARL SIEMENS, M.I.C.E.

Members of Council.

PROFESSOR W. G. ADAMS, F.R.S.
CAPTAIN ANDERSON, R.E.
CAPTAIN ARMSTRONG, R.E.
G. VON CHAUVIN.
H. G. ERICHSEN.
COLONEL GLOVER, R.E.
EDWARD GRAVES.
CHARLES HOCKIN, M.A., C.E.
LOUIS SCHWENDLER, *Member of Council of Asiatic Society.*

WILLOUGHBY SMITH.
C. E. SPAGNOLETTI, M.I.C.E.
C. F. VARLEY, F.R.S., M.I.C.E.

Associates of Council.

FREDERICK HIGGINS.
H. R. KEMPE.
E. T. TRUMAN.

It was announced that the Council had transferred the following gentlemen from the class of Associates to the class of Members.

E. C. WARBURTON.
G. G. WARD.
JOSEPH OPPENHEIMER.
ANDREW JAMIESON.

The following Candidates were balloted for and declared duly elected :—

FOREIGN MEMBERS :—

R. K. Boyle.
Don Alexandro de Bejar.
George D'Infreville.
Don José de Redonet.
Carl Vogel.

MEMBERS :—

Adam Armstrong.
Captain Fred. Harvey.
E. Houghton.
F. G. Maclean.
G. J. Moberly.
J. G. Pope.
Earl Poulett.
M. R. Trower.
A. O. Walker.
Cecil Wray.
Leonard Wray, jun.

Associates:—

J. O. L. Berner.
J. Probert.
J. G. Trott.
W. T. Tunbridge.
T. B. Webber.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

NOTES ON ELECTROLYTIC POLARISATION.

By Professors W. E. Ayrton and John Perry,

Professors at the Imperial College of Engineering, Tokio, Japan.

From December 1875 to August 1876 we made several experiments on polarisation phenomena. An account of the preliminary work will, we hope, be found of some interest.

Two platinum plates, each 10 by 8 centimètres, were placed parallel at 21·3 centimètres apart in a mixture of pure water and sulphuric acid (specific gravity 1·016 at 50° F.). These plates could be connected by a key with the poles of a Daniell's cell, the resistance of connections, galvanometer shunted, and of the cell, being about 8 ohms. The current flowing through these connections was measured in farads. The platinum plates were permanently connected with the electrodes of a quadrant electrometer. When the current was broken the plates were left insulated, and subsequently connected by resistance coils and a very delicate galvanometer.

We arranged a break-circuit-chronograph, using a Morse ink-writer, worked by the laboratory clock, to record the time when taking readings of the rapid diminution of current and of difference of potentials.

We would here say a few words about the measurement of a deflection when it is decreasing very rapidly. Supposing that there is a constant deflecting couple acting on a vibrating body which is swinging, with decreasing swings, about its position of ultimate rest, we know that its motion may be calculated if we imagine that it is eveywhere retarded by a force varying as the velocity of the body (see Thomson and Tait's Elements of Natural

Philosophy, articles 294-6, and Clerk Maxwell's Electricity, articles 730-51). Thus in a reflecting galvanometer or electrometer, x is the distance at the time, t of the moving spot of light from a fixed point O of the scale, and if a is the reading of the position of rest of the spot then the equation of motion is

$$\frac{d^2x}{dt^2} = \mathrm{K}\,(a - x) + \mathrm{K}'\,\frac{dx}{dt},$$

where K is a constant depending on the moment of inertia of the needle, &c., and K′ is a constant depending on the viscosity, &c. of the surrounding medium. Now, when we suppose that the current or difference of potentials is constant during the swinging, that is that a is a constant, it is easy to solve this differential equation, a solution being

$$x - a = c\epsilon^{tc'}$$

where c and c' are constants. If x_1, x_2, x_3, &c. are the readings of successive elongations (when we speak of an elongation we mean the reading of the limit of the swing), then we may begin to count time from (say) the first elongation, and if T is the half time of a complete oscillation of the needle

$$\begin{aligned} x_1 - a &= c \\ x_2 - a &= c\epsilon^{-\mathrm{T}c'} \\ &\vdots \\ &\vdots \\ x_n - a &= c\epsilon^{-(n-1)\mathrm{T}c'}. \end{aligned}$$

And given x_1, x_2, and x_3, we find from the first three of the above equations that

$$a = \frac{x_1 x_3 - x_2^2}{x_1 + x_3 - 2x_2}$$

Now, suppose we know a the position of rest of a needle which we have set swinging, then taking the elongations x_1, x_2, and x_3, we see that if the needle obeys in its swinging the above law, then

$$(x_1 - a)(x_3 - a) = (x_2 - a)^2.$$

This is a test which an experimenter ought to apply to his instrument so as to find to what extent he may apply the formulæ, and he will see that when the test is satisfactory the period of the swing is approximately constant. If, as is often the case, the swings are

very great, then certain corrections ought to be applied because of the swings not being isochronous.

Now, if we take time readings of the elongations, or extremities of swing, of (say) a galvanometer needle when the current is altering, we find no help in any mathematical investigation hitherto published to enable us to measure the real current at any instant. In the absence of any exact method we have assumed what is nearly true when the charge in a is not too rapid, that if a is the true deflection at the time when the spot is at x, then

$$x - a = c\,\epsilon^{-tc'}.$$

It is difficult to see what better approximation can be made to the solution of this differential equation, but certainly c varies somewhat when a varies. If when no current is passing we cause the needle to swing to right and left of zero, two successive swings from zero being X_1 and X_2 opposite in sign, and if, as before, T is half the time of a complete vibration, which remains nearly the same in all cases, then

$$\epsilon^{Tc'} = \frac{X_2}{X_1}.$$

And hence we may say that if a_1, a_2, &c. are the true deflections when the spot of light is at the limit of its swings x_1, x_2, &c. then

$$x_1 - a_1 = c$$

$$x_2 - a_2 = c\frac{X_2}{X_1}$$

$$x_3 - a_3 = c\left(\frac{X_2}{X_1}\right)^2$$

$$\vdots \qquad \vdots$$

$$x^n - a^n = c\left(\frac{X_2}{X_1}\right)^{n-1}$$

Now, let the times of successive elongations of a vibrating spot of light be taken, and let the readings and the times be shown on the points 1, 2, 3, &c. (fig. 1), time being measured parallel to OT. Now draw a curve through the points 1, 3, 5, 7, &c., and another through 2, 4, 6, 8, &c. Draw any number of ordinates bounded

by the two curves and bisect them in the points A, B, C, &c., then if the equation

$$x = a + c\epsilon^{-tc'}$$

(where c is a constant) be accepted as a sufficiently accurate solution of the differential equation given above, the curve joining A, B, C,

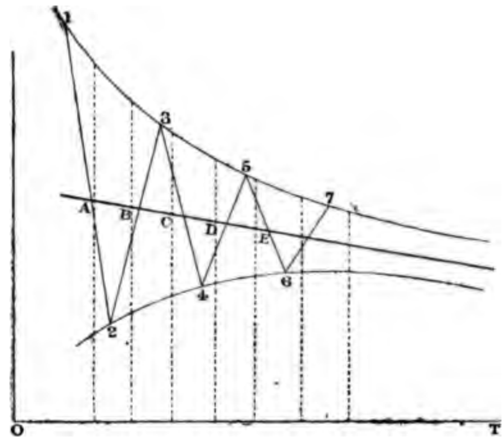


Fig. 1.

&c., the middle points of the ordinates show the time readings of the true deflections. To prove this:—Let time be counted from the first elongation, point 1 in the above figure, and let $a = f(t)$, let $-\frac{H_2}{H_1} = K$, so that K is always positive and is the ratio of the numerical values of the swings to right and left of a constant zero, then

$$x_1 = f(O) + c$$
$$x_2 = f(T) - cK$$
$$\cdot \qquad \cdot$$
$$\cdot \qquad \cdot$$
$$\cdot \qquad \cdot$$
$$x^n = f\{(n-1)T\} \pm cK^{n-1}$$

Now $x = f(t) \pm CK^{\frac{t}{T}}$ is the equation to a curve passing

through all the points 1, 2, 3, 4, &c., $x = f(t) + \mathrm{C}\,\mathrm{K}^{\frac{t}{\mathrm{T}}} = \phi\, t$ (say) is the equation to some curve passing through the points 1, 3, 5, 7, &c., and $x = f(t) - \mathrm{C}\,\mathrm{K}^{\frac{t}{\mathrm{T}}}$ is the equation to some curve passing through the points 2, 4, 6, 8, &c. Let us first consider—

$$x = f(t) + \mathrm{C}\,\mathrm{K}^{\frac{t}{\mathrm{T}}}$$

$$\frac{d\,x}{d\,t} = \frac{d\,f}{d\,t} + \mathrm{C}\,\mathrm{K}^{\frac{t}{\mathrm{T}}} \times \frac{1}{\mathrm{T}} \log.\ \mathrm{K} \quad \ldots\ldots\ldots \quad \text{(I.)}$$

Now, since the current passing through the galvanometer is supposed to be steadily diminishing it follows that $x = f(t)$ must be a curve without waves, something like fig. 2.

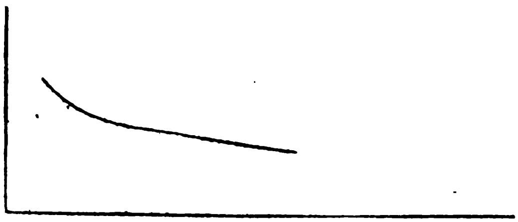


Fig. 2.

therefore $\frac{d\,f}{d\,t}$ is always negative; and since C and T are both positive, and log. K is negative, being the logarithm of a proper fraction, it follows that $\frac{d\,x}{d\,t}$ must be always negative, and therefore can never be nought. And since neither of the terms on the right hand side of equation (1) can ever be infinite $\frac{d\,x}{d\,t}$ can never be infinite.

Again, $\frac{d^2\,x}{d\,t^2} = \frac{d^2\,f}{d\,t^2} + \mathrm{C}\,\mathrm{K}^{\frac{t}{\mathrm{T}}}\left(\frac{1}{\mathrm{T}} \log.\ \mathrm{K}\right)^2$, and from the general appearance of fig. 2 we see that $\frac{d^2 f}{d\,t^2}$ is always positive and finite also. $\mathrm{C}\,\mathrm{K}^{\frac{t}{\mathrm{T}}}\left(\frac{1}{\mathrm{T}}\log.\ \mathrm{K}\right)^2$ must be always positive and finite, there-

fore $\dfrac{d^2 x}{d t^2}$ can never be nought nor infinity; therefore

$$x = f(t) + C K^{\frac{t}{T}}$$

is the equation of a regular curve without points of contra reflexia and without points, at which x is a maximum or minimum, therefore it must be very like the one we have drawn through 1, 3, 5, 7, &c., in fig. 1.

For the same value of t,

$$x = f(t) + C K^{\frac{t}{T}} = \phi t$$

$$\text{and } x = f(t) + C K^{\frac{t}{T}} = \psi t$$

$$\therefore f(t) = \frac{\phi t + \psi t}{2}$$

But $x = f t$ is the equation of some regular curve, so also (as we have proved) is $x = \phi t$, therefore $x = \psi t$ must also be the equation of a regular curve through 1, 3, 5, 7, &c., and consequently must be something like the one we have drawn in fig. 1. Therefore, a regular curve drawn through A, B, C, D, and the middle points of the differences of the ordinates must be the curve $x = f(t)$, and therefore represents the true position of equilibrium of the spot of light at any moment.

Curves A B, A′ B′, C C′.

In fig. 3 time is measured parallel to the line O X, starting

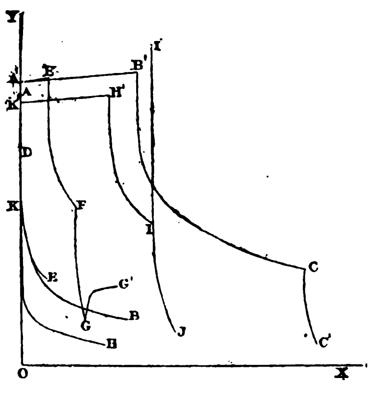


Fig.

from O, which represents the moment of closing the circuit; galvanometer and electrometer deflections are measured parallel to O Y. Curves A B represent the time-fall of battery-current from 0·001080 farads per second, at 15 seconds from the closing of the circuit, represented on the point A, to 0·000174 farads at 46 minutes, represented in the point B. The current during the first quarter of a minute seemed to be many times greater than the ordinate at A, as if the voltámeter during that short time had a very small resistance indeed. This was observable in all the experiments, the current during the first short interval of time being very much greater than any which we were able to measure.

During the first 46 minutes the electrometer reading increased from 114·1 to 115 represented by the ordinates of the line A′ B′. At the end of 46 minutes the platinum plates were suddenly insulated (except that there was the acidulated water of the voltameter between them), and the time-fall of the electrometer readings is shown in the curve B′ C. At C (112 minutes from the beginning of the experiment) the plates were connected through a resistance of 12,000 ohms, and the curve C C′ shows the electrometer readings to the end of this experiment. The current through the resistance-coils was not observed.

Curves D E, A′ E′, F G G′.

The battery current through the voltameter fell from D, the first observation which we were able to make, to E, corresponding to 0·000321 farads per second, in 11 minutes from the closing of the circuit. During these 11 minutes the electrometer reading increased from A′ to E′. At E or E′ the plates were insulated, and E′ F shows the fall of electrometer reading. At F (22 minutes after the commencement of the experiment) the plates were connected through a resistance of 12,594 ohms; the consequent fall of the electrometer is shown to G (3¾ minutes from F). At G the plates were again insulated. The time-rise of the electrometer to G′, the end of the experiment, is very noticeable, and is of great interest. The shape of this curve G G′ strikingly resembles the shape of the curve of rise of potential (due to soaking out) of a telegraph cable or Leyden jar that has been kept charged for some

time, then suddenly discharged and insulated. More light has been thrown upon this resemblance in more recent experiments made by us.

Curves K H, K′H′I, I′J.

The liquid in which the platinum plates was immersed was now a mixture of distilled and ordinary water, no acid being added, the dimensions, &c. of the platinum plates remaining as before. K shows the first galvanometer reading about which we have any certainty; it represents a current of 0·000632 farads per second at 12 seconds from closing the circuit. This reading was obtained (see our method given above) from the second, third, and subsequent extensions of swing of the needle. The first swing was extremely great, so that the fall of current during the first 12 seconds must have been exceedingly great. The current falls to H 0·000087 farads per second at 34½ minutes from beginning of the experiment. At H the plates were insulated. K′H′ represents the rise of electrometer readings during the first 34½ minutes. At H′ the reading is almost exactly the same as that of the Daniell's cells employed in this experiment. H′I represents the fall of electrometer readings, the plates being insulated. At I (52 minutes from the beginning of the experiment) the plates were connected through coils, having a resistance of 10,000 ohms, and the current was measured. The time fall of this current is partly shown in the curve I′J. This curve shows current on a scale 123 times as great as the scale of the other curves on the sheet AB, DE, KH, but the scale for time remains unchanged. The first reading which we were able to determine with any accuracy was 0·00013 farads per second at 11 seconds after connection, and the swings of the needle during these 11 seconds were exceedingly great.

Experiments having been made on polarisation with platinum plates, when the resistance external to the voltameter was very small compared with that of the voltameter itself the platinum plates were replaced by clean copper plates, to see in what way the results would be affected. It was found that while in a particular case the current passing from one Meidinger cell through a voltameter, consisting of two platinum plates immersed in ordinary

water, was diminished 90 per cent. in nine minutes; the current from the same cell passing between two clean copper plates at the same distance apart as the platinum plates, and immersed to the same depth in the water, was only diminished 9 per cent. in 30 minutes, and the copper plate attached to the copper pole of the battery was browned. With two iron plates the diminution of current was also very slow. The copper plates were then platinised with platinum tetrachloride, but the rate of diminution of the current was not found to be much increased; this was possibly partly due to the tendency the platinum had to run off. A copper and a platinum plate were then tried, and it was found that when the copper plate was the anode (so that oxygen would have been deposited on the copper plate were the water decomposed) the polarisation was practically the same as in the case of two clean copper plates, but when the battery was reversed, the copper and platinum plates in the voltameter having been previously thoroughly cleaned, then the falling off of the current was (roughly) about as rapid as in the case of two platinum plates. Next two copper plates were employed, the one quite clean the other thoroughly oxidised by previous electrolysis, and it was found that when the clean copper plate was used for the cathode, and the oxidised one for the anode in the voltameter, the polarisation was very rapid, hence showing that an oxidised copper plate acted nearly as well as a platinum plate for the anode. Starting with these results we made a number of experiments to find out whether the polarisation between two thick copper wires, one of them oxidised and the other clean, lying near one another for about one hundred yards in the College Moat, was sufficiently great to produce any considerable surface resistance, such as would lead to a new method of submarine signalling. For such a scheme to prove successful the polarising battery must have a constant electromotive force about equal to that of a Daniell, and it must have a very small internal resistance consequently; for the above experiments with the long wires, Thomson's tray cells, each having plates of about three square feet area, were employed, with receiving instruments of very small resistance.

The results obtained were satisfactory, but want of time, and the necessity for observations on a larger scale, have compelled us to

postpone these experiments with the long wires for the present, and in the meantime we have been making others, which have, however, a direct bearing on the preceding.

Up to the present time our observations had been confined to plates, of which the linear dimensions were not large compared with their distance asunder. We now made experiments with two platinum plates placed vertically in ordinary water parallel to one another, and about eight inches asunder; the immersed area of each side of each plate was about one and a half square feet. The diminution of current was in all the cases we tried comparatively small, and we append a list of the batteries employed, arranged in order of electromotive force.

Battery.	Reduction of current.
Three batteries in series, each of the first two being five Grove's cells joined parallel, and the third consisting of six bichromate of potash cells joined parallel	3·6 per cent. in 36 minutes.
Two batteries in series, each consisting of five Grove's cells joined parallel	3·8 per cent in 13 minutes.
Ten Grove's cells joined parallel	11·6 per cent. in 34 minutes.
Six old bichromate of potash cells joined parallel	16 per cent. in 1 hour.
One battery consisting of ten Grove's cells, and six bichromate of potash cells, all joined parallel	7·3 per cent. in 43 minutes.
One porous cell (Daniell)	Inappreciable.

With the exception of the fourth battery, the six old bichromate of potash cells, we find that the amounts of the diminutions shown above are, when calculated for equal times, roughly speaking in the order of the numbers obtained by dividing the electromotive force of the battery by the battery resistance.

The current which flows into a voltameter is employed first to supply the loss of polarisation by dissipation (usually supposed to be a current through the voltameter); secondly, to increase the polarisation. As the polarisation increases, a greater proportion of the current is employed in supplying loss than in increasing polarisation, so that, if on account of the size of the plates, their distance asunder, &c., the rate of loss be very great, there will be scarcely any increase of polarisation or diminution of current.

Now this loss by dissipation does it occur between one of the plates and the other, or between different parts of the same plate, or is it due to a combination of these causes? The following experiments throw light on this point; H and O (fig. 4) are bell

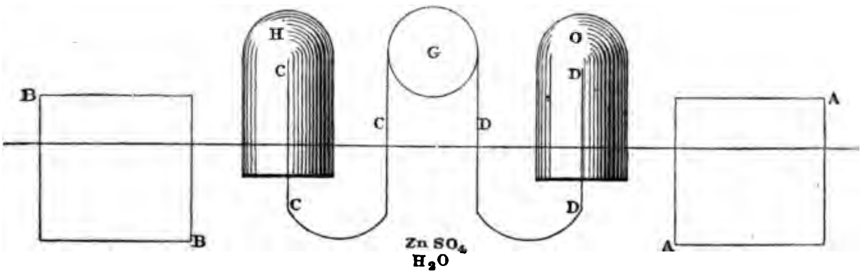


Fig. 4.

jars containing hydrogen and oxygen inverted in an extremely weak solution of zinc-sulphate, C and D are bent platinum plates connected with the terminals of a galvanometer G. A and B are large platinum plates partially immersed in the dilute zinc-sulphate, and which could be touched to D or C; A and B had been previously kept connected with one another for a long time to bring them to the same electric state.

The following tests were made:—

1. The current produced by C and D was measured . . . . . . = + 15
2. A connected with D, and the current produced by C and D + A, measured . = + 80
3. B connected with C and the current produced by B + C and D measured . — + 55

4. D disconnected from the galvanometer and the current produced by C and A measured . . . . = + 62
5. D reconnected with the galvanometer, but C disconnected, and the current produced by B and D, measured . . = − 62

The next experiments were made to examine the action of C and D, when, instead of being placed in the hydrogen and oxygen, they had gases deposited on them electrolytically, in which case the direct current from C to D would be much more energetic.

CC′, DD′ (fig. 5), are two platinum plates immersed in very dilute zinc sulphate, and which may be connected directly with a Minotto's cell, or with a galvanometer G and resistance coil of 10,000 ohms.

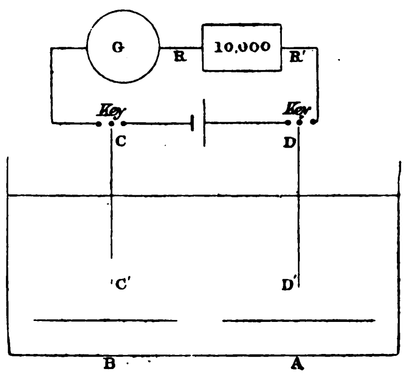


Fig. 5.

A and B are two platinum plates which were kept under water in a vacuum for some time to partially remove any gas that might be condensed on them, and they were placed at the bottom of the zinc-sulphate without exposure to the air. When CC′ and DD′ were connected with the Minotto's cell, DD′ was the anode, CC′ the cathode. We shall not, at present, speak of any phenomena of charging, but only of those that occurred after removal of the battery and connection with the galvanometer and resistance coils. The current from D to C was found to be many times increased when A was connected to D by means of a platinum wire, or when

connected with the point R, and when A, instead of being connected with D, was connected with C′, the current was reversed. In all cases the current diminished with considerable rapidity. The same phenomena were observed when the experiment was repeated with B instead of A. This set of experiments, therefore, confirms the results previously obtained, when the gases in contact with the plates were not deposited electrolytically. Both A and B were of course always disconnected from DD′ and CC′ when these latter plates were being polarised with the Minotto's cell. Continued repetition of the experiments made the results less and less marked, as was to have been expected, since A and B were no longer in their primary neutral state.

When five Minotto's cells in series were employed instead of one to produce polarisation, it was found that the current from D to C was diminished when A was joined by a platinum wire to D, or when B was joined by a platinum wire to C, and a still further diminution was produced if these connections were made simultaneously. If, however, B was connected by means of a platinum wire with R then there was a great increase of current. When D was disconnected from R′, and a platinum wire connected with R merely dips into the liquid, there was a current which was many times increased when this wire touched A or B. Similar results were obtained when the galvanometer was placed between D and R′, D connected with the galvanometer, A disconnected and the platinum wire connected with R′.

It is possible that the reason why some of the results obtained when CC′, DD′ were polarised with five cells apparently differed from those obtained when one cell was employed may have been due to the plates A and B not being in their primary neutral state, since they were not heated nor placed in a vacuum between the two series of tests with one Minotto's cell and with five. It was not, however, thought advisable to repeat the above tests, but rather to make more accurate ones with an improved form of apparatus which the above tests showed the desirability of constructing.

In the meantime experiments were carried out to examine how variation of external resistance and of external electromotive force

would influence the polarisation of platinum plates immersed in ordinary water. The plates were the same, and at the same distance apart, as in the first experiments of all described at the beginning of this paper. Curves I. III. IV. and VI. and IX. (fig. 6) show the time increase of the electromotive force of polarisation produced by a battery composed of two old Minotti's cells, the external resistances in circuit, excluding that of the voltameter, being respectively 10,000, 5,000, 2,000, 1,000 and 0 ohms; thus in curve I. the electromotive force of polarisation at the end of five minutes from the starting of the polarising current is shown by the ordinate AB, the time being denoted by the distance OA measured from O.

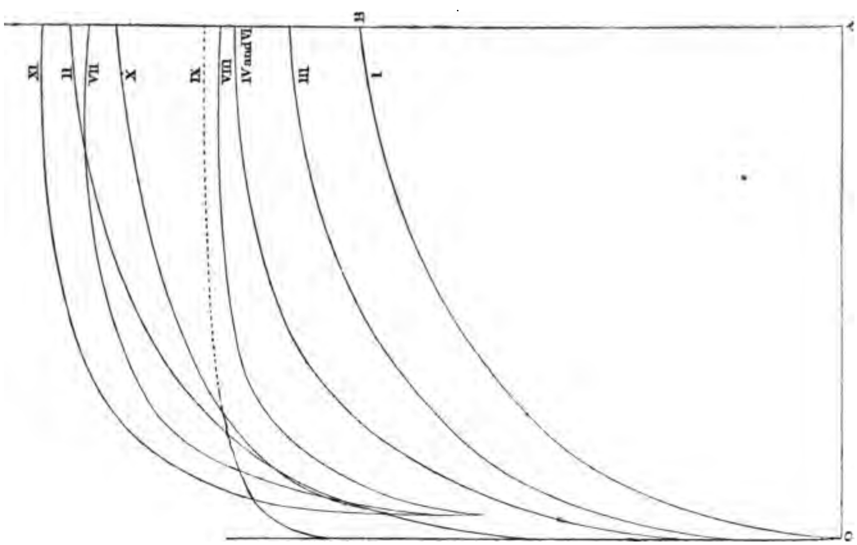


Fig. 6.

The method of making the experiments was simple: A coil of known resistance, together with a delicate reflecting Thomson's galvanometer, being inserted in the circuit with the battery and voltameter, the difference of potentials of the platinum plates was continuously measured by the deflection of a Thomson's quadrant electrometer, with the electrodes of which they were kept connected. Let V be the difference of potentials of the platinum

plates at any moment, as measured by the electrometer, and let G be the galvanometer reading at the same instant; let E be the constant electromotive force of the battery and R all the resistance external to the voltameter, being composed, of course, of the resistance coil together with the resistances of the battery and galvanometer; then

$$\frac{E - V}{R} = K G,$$

where K is a constant which can be determined from any pair of simultaneous observations of galvanometer and electrometer, the constancy of the value of K determined from different pairs of observations being in fact a test of the accuracy of the experiments. Certain discrepancies which we observed in the somewhat different values of K thus obtained may undoubtedly be accounted for by the yielding of the silk suspension fibres in the electrometer described by us in a paper to the Society of Telegraph Engineers.* The appearance of the above curves is not much, however, affected by these discrepancies, which appear to have much increased in curves II. VII. X. and XI. described further on. K having been calculated, we can determine the value of E, the electromotive force of polarisation, as follows:—Let us assume the resistance of the voltameter to remain constant, and want of knowledge on the subject of polarisation makes such an assumption necessary for the present (in our particular voltameter direct tests with a Wheatstone's balance showed the resistance to be about 900 ohms), then V and G having the same meanings as before,

$$\frac{V - e}{900} = K G$$

or

$$e = V - 900 \, K \, G.$$

Curves IV. and VI. each being the curve for an external resistance of 2,000 ohms, are drawn as one curve since the results of two successive experiments gave nearly identical results. Curves II. VII. X. XI. are the results of similar experiments made with two new porous cells, Daniell's, in series, when the resistances external to the

* See page 481.

voltameter were respectively 5,000, 1,000, 0,500 ohms. In addition to the yielding of the electrometer suspension fibres, causing discrepancies in the value of K, it is probable that the assumption just made of the constancy of the voltameter resistance may also be another cause of possible error in the values of *e*. The electromotive force of the two Minotto's cells previously used was about three-quarters of that of the two porous cells, Daniell's, used for curves II. VII. X. and XI. If P be called the maximum electromotive force of polarisation obtainable with a large battery, then P is greater than E in the case of the two Minotto's cells, nevertheless the maximum value of *e* with the Minotto's cells is less than E, except perhaps in curve IX. and this is probably due to what may be called the dissipation of polarisation being as great as the supply afforded by the battery when the maximum is approached. The curves show in a very marked way why the charging of a voltameter as a condenser is so very different from the charging of a glass condenser, this difference being due (first) to the much greater dissipation in water than in glass, and (secondly) to the resistance of the battery galvanometer and connections being comparable with the resistance of the voltameter, which never happens in the case of a glass condenser, consequently curve IX. more nearly corresponds than any of the others with the curves, showing the production of internal or opposing electromotive force in a glass jar, which have been drawn by us from experiments performed with the flint glass jar of a Thomson's electrometer, and which will shortly be published.

The greater approximation towards the right hand side of the figure 6 in the case of curves II. VII. X. XI. than in the case of corresponding curves for the two Minotto's cells, is due probably to the electromotive force of the battery in the former case being greater than the maximum obtainable electromotive force of polarisation. The defects in the electrometer being more perceptible, the higher the difference of potentials to be measured may cause the relative appearance of curves II. VII. X. XI. to be more disturbed than in the case of the other curves. We have no doubt, also, that with a very accurate electrometer all the curves would be steeper at the beginning.

The following experiments are a continuation of those previously described, made of the discharge between a polarised and a neutral plate. O, H, S (fig. 7) are three platinum plates immersed symmetrically in a large glass vessel containing *distilled* water. The immersed area of each side of each plate was about 60 square centimètres and the resistance between any two of the plates was 24,000 ohms in the earlier experiments, and 30,000 ohms in the latter ones when the water was hydrogenated. In each of the six

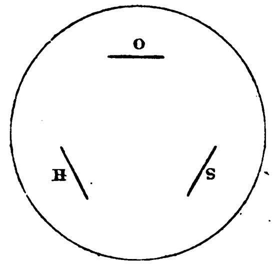


Fig. 7.

experiments O and H were polarised with a new porous cell, Daniell, for six minutes, O being the anode, H the cathode, and S kept at potential nought, and having no connection with the battery circuit except in so far as it was in the liquid; the battery circuit not being connected with the ground, no current passed through it during charging. At the end of the six minutes the battery was disconnected and two of the plates were instantly connected through the poles of a Thomson's delicate reflecting galvanometer with an additional resistance of 10,000 ohms in circuit to observe the dissipation of polarisation. Between each set of experiments all three plates were short-circuited until they would produce no current through the galvanometer. Curves A, B, C (fig. 8) show time readings of the current in three successive chargings, the liquid being ordinary water, time being measured horizontally along OX, OL being equal to six minutes, and current measured vertically,

LA, measured along a line through L at right angles to OX, representing a current of 14·1 microfarads per second flowing through the voltameter.

a shows the discharge current between				O and H	
b	,,	,,	,,	O ,, S	
c	,,	,,	,,	S ,, H	

The water being thoroughly hydrogenated by bubbling hydrogen gas through it for a considerable time, the experiments were re-

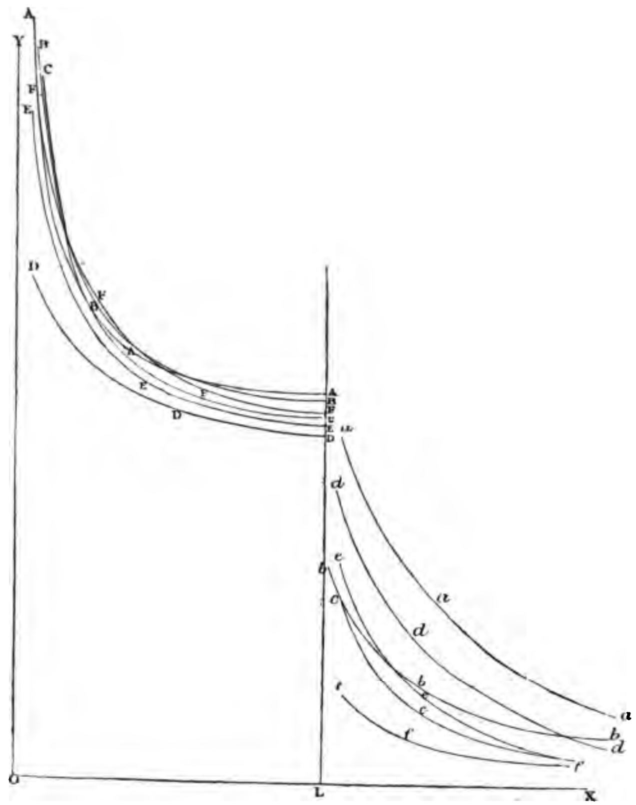


Fig. 8.

peated; D, E, F show the three charging currents obtained in the three successive cases

d	,,	,,	,,	O and H
e	,,	,,	,,	O ,, S
f	,,	,,	,,	S ,, H

In all cases the points A, B, C, D, E, F, nearest O Y, or the points *a*, *b*, *c*, *d*, *e*, *f*, nearest L A, are the first observations that could be obtained with accuracy.

One of the most striking features in these curves is the extreme smallness of the ordinates of *f*, the curve showing the discharge current between S and H in thoroughly hydrogenated water. It therefore appears as if the cathode, even when the polarising currents is only produced by one Daniell, behaves as if it were really coated with hydrogen, and the discharge current between a charged and a neutral plate is due to the re-absorption of the gas by the water, the rapidity of which absorption we might expect to proceed very slowly in the case of a liquid, saturated with the same gas as is condensed on the charged plate. Similar considerations explain the general appearance of all the discharge curves.

Continuous electrometer readings were also taken both during charging and discharging. They do not, however, supply any information not obtainable from the galvanometer readings, since the difference of potentials of the platinum plates during charging was necessarily constant since the resistance external to the voltameter was extremely small in comparison with that of the voltameter itself, and the difference of potentials during discharge was so small as to render the drawing a curve from the electrometer readings during discharge impossible. This smallness of the difference of potentials of the platinum plates during discharge arises not only from the comparative rapidity of dissipation of polarisation, but also from the resistance of the voltameter being more than twice the resistance external to it during discharge.

For the purpose of carrying out these investigations in gas, freed liquid, or in a liquid saturated with any special gas, the following apparatus (fig. 9) was constructed. A vessel AB, 14 centimètres long and 4 centimètres in diameter, was connected at the bottom with a vertical glass tube BC, to which was joined a long india-

rubber tube MC, connected with a mercury reservoir M, open to the air, and which could be lowered into a position M′ for producing a vacuum in AB, as in a Geissler's vacuum pump. AB was closed with an air-tight cork AD, through which passed a tube EF, which could be tightly closed at the upper end. Through AD passed also six thick copper wires, of which four are shown in the drawing *a*, *b*, *c*, *d*. To the ends of each pair were joined the ends of a platinum spiral, made of fine platinum wire one foot long, so that each of the platinum plates in our last experiment was now replaced by a platinum spiral. From E to G the space was filled with distilled water, and from G to M the space GBCM was filled with mercury. When E was closed and M lowered to M′, the level of the water fell to H, and a good vacuum was produced in EH. The platinum spirals were then made red-hot in the vacuum, by passing a strong current from *a* to *b*, *c* to *d*, *e* to *f*, so as to free them perfectly from gas, which mere exposure in a vacuum, even for a long time, will not do. It is in this particular that this apparatus is an improvement on that employed by Prof. Helmholtz in his experiments on polarisation, the importance of which addition will be seen from the results we obtained and which are now about to be described.

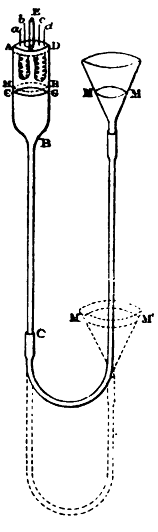


Fig. 9.

A and B (fig. 10) show the charging curves obtained with one Daniell's cell when the distilled water was perfectly freed from air, and the platinum spirals keep some days in the vacuum EH, *but not heated.* Current is measured parallax to OY and time parallel to OX, each reckoning from O; OY represents a current of 0·0000097 farads per second passing through the voltameter and OX a time of eight minutes.

a b represent the curves of discharge when the battery is removed and the plates or rather spirals short-circuited through a

Thomson's delicate reflecting galvanometer with a resistance coil introduced in the circuit, the total resistance exterior to the galvanometer being 31,000 ohms, time being reckoned from O, the instant of discharge along O A, the scales for time and current remaining as before.

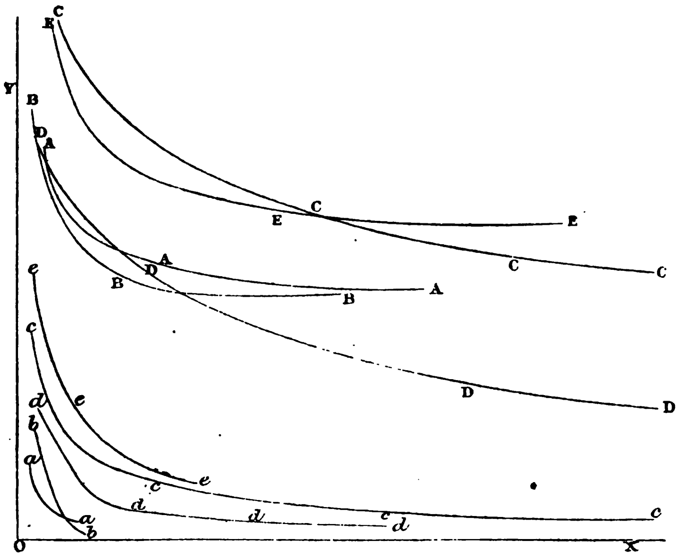


Fig. 10.

C and *c* are similarly obtained curves when the distilled water was thoroughly freed from gas and the platinum spirals were in addition made red hot; D and *d* when the water and spirals were thoroughly aerated.

E and *e*, when the water and platinums were thoroughly freed from air, but thoroughly saturated with hydrogen.

LIGHTNING CONDUCTORS.

In No. xii. vol. iv. of the Journal of the Society of Telegraph Engineers, there is a letter by Professor Clerk Maxwell on the interesting question of lightning conductors. His views are more fully given in a paper read before the British Association, and published in *Nature*, September 28th, 1876. He states that it is not necessary to have any earth-connection, or to have a tall conductor with a sharp point, but that a connected system of wires laid along the edges of a building will sufficiently protect it from the effects of a lightning discharge. It seems at first sight that the reasoning is correct, based as it is on the fact that when electricity is *at rest* every point inside a conductor containing no electrified bodies is at the same potential as the conductor itself, and also that even a cage, of which the wires are widely separated from one another, behaves like a complete metal covering in its screening action. But we think no theory deduced from static phenomena can be regarded as satisfactory when applied to the induction phenomena of a lightning discharge. The discharge of a cloud, whether to other objects or to Professor Clerk Maxwell's cage, is disruptive, and must be regarded (*see* Clerk Maxwell's "Electricity," art. 231) as the commencement, passage, and end of a current of electricity; and since, as is well known, no conducting cage will screen a circuit from the effects of current induction, it is evident that an instantaneous current may pass between two points inside a cage during a lightning discharge in the neighbourhood. Looking at the subject from a static point of view, we see that with a given electric distribution A outside a cage C there is always such a distribution over C as keeps two points, B and D (supposed to have no absolute charges), at the same potential. If now there is a charge in A, a charge will be produced in C, and B and D will be again at the same potential. But we would ask if there is any evidence to prove that during this charge the conditions outside and inside the cage are such as to keep all points inside at the same potential. Indeed the charge in C is a consequence of the alteration in A, producing a certain

distribution of potential over all points in space, and this affects B and D as well as C, consequently a current may take place from B to D just as well as from one point of C to another, an effect which would be very marked if B and D were joined by a silver wire, and if the material of C were not a good conductor; and even if C be a moderately good conductor the current will take place from B to D even across a certain thickness of air if the almost instantaneously produced difference of potentials be sufficiently great.

The circumstances under which current induction usually takes place, say in a Ruhmkorff coil, show that the screening action of a cage is inconsiderable, and certain experiments that we made some few months ago in connection with an investigation of the magnetic transparency of bodies may be mentioned here as illustrative of the extreme smallness of this screening action.

I. A coil $3\frac{1}{2}$ centimètres in diameter and $2\frac{1}{2}$ centimètres thick, of very fine silk-covered wire of 185 units resistance, wound on a very thin wooden bobbin, was placed with its axis coinciding with that of another exactly similar bobbin at a distance of 6 millimètres asunder; a current from 80 Daniell's cells was sent through one coil and suddenly reversed, and the current induced in the other coil measured by a very delicate reflecting galvanometer (constant of galvanometer one Daniell's cell through 30,000 ohms with the $\frac{1}{1000}$th shunt) gave a deflection of 600 scale divisions). The current induced in the second coil was found not to be altered by one-half per cent. when a sheet of copper of 225 square centimètres area and one millimètre thick was introduced between the two coils so as to completely hide the one from the other.

II. One of these coils was suspended by a fibre of unspun silk, with the two ends of the wire dipping into mercury cups, placed one under the other in the axis of rotation of the coil. A mirror attached to this coil produced an image on a scale 75 centimètres distant. The first operation was to render this coil when traversed by a strong current astatic as far as the earth's magnetism was concerned; this was of course arranged by means of a suitable adjusting magnet. The second coil was now fixed at a distance of sixteen centimètres from the first, and joined up in circuit with it. The

deflection produced in the suspended coil by the stationary coil was now accurately measured, and it was found that there was no apparent alteration in this deflection, certainly not an alteration of one per cent., by the introduction of a solid rectangular block of copper 30 centimètres long, 15 centimètres thick, and 15 centimètres high, between the coils, so as to fill up the entire air space between them, and so that one coil was completely hidden from the other. In fact the frequent slow interposition and removal of the copper block, which was placed on a carriage with wooden wheels for this purpose, produced no visible effect in the action of one coil on the other.

III. The secondary coils of two induction arrangements were joined up in circuit with a very delicate galvanometer in such a way that the induced currents balanced one another and so that the same current traversed each primary coil. The one induction arrangement was an ordinary Ruhmkorff's coil, the other consisted of two horse-shoe electro-magnets, placed end to end; the core of each of these horse-shoe magnets was 8 millimètres in diameter, and each leg of each magnet was furnished with a bobbin of wire 3 centimètres in diameter, the length of each bobbin being 5 centimètres, and the resistance of each electro-magnet 300 ohms. Now it was determined with this delicate differential arrangement that the current induced in one of these electro-magnets by a sudden reversal of the current in the other was not altered by $\frac{5}{100}$ths (0·05) per cent. when a sheet of copper, brass, or zinc, 225 square centimètres in area, and one millimètre thick, was interposed between the two electro-magnets so as to completely hide the one from the other. Practically, therefore, current induction is not in the least diminished by the interposition of a screen of copper, brass, &c. Hence in the case of a building surrounded by a cage, but unprovided with an elevated point and an earth-plate, since, when disharges are taking place into the cage or in its neighbourhood these discharges are disruptive, the cage will have no effect in preventing an induced disruptive discharge inside; in fact the only way of rendering an internal disruptive discharge impossible is by preventing all disruptive discharge either near the house on the outside or through the metallic or other conductors of the house.

Now we wish to point out that with the common method of protecting a building from lightning when the point taps the cloud for the benefit of the building and all bodies in its neighbourhood, it performs this operation slowly, and disruptive discharge is impossible whether we imagine that a cloud comes from a distance and discharges itself at the point or that the point and the earth are subjected to a return shock. It also performs efficiently the office of the proposed cage, and is, therefore, a more efficient protector. It is probable, however, that, since the point requires to be sharp and the earth-plate in good contact with the earth, any ordinary lightning-rod will gain in efficiency by being supplemented by a cage in metallic connection with it.

We have been led to make the above remarks by our knowledge that although perhaps a metallic cage may be a good protection to a powder-mill it is not a perfect one, and that even a complete cage would not, as Professor Clerk Maxwell's letter might lead one to suppose, be absolutely satisfactory, also a cage whether complete or not is as good a protector as a well-constructed lightning-rod.

Professor Clerk Maxwell remarks that with a cage a house may be insulated from the ground by a layer of asphalte. But if a house be well insulated by a very thick layer of dry asphalte, then, neglecting the inductive effects to which we have referred in this paper, and which are not affected by the absence or presence of a cage, it is quite immaterial whether there is a cage or a lightning-rod, or any protector of any kind, since such a house is from the fact of its perfect insulation quite safe from damage by lightning.

W. E. Ayrton.
John Perry.

The Imperial College of Engineering,
Tokei, Japan.

EXPERIMENT TO SHOW THE DIRECTION IN WHICH THE ELECTRIC SPARK TENDS TO TRAVEL.

If into the secondary circuit of a Ruhmkorff's induction coil we introduce an extremely high resistance, such as that due to an inch of dry air, we shall find, whether sparks pass across this interval of resistance or not, that the conductors on either side are electrified in opposite directions by a series of rapid pulsations.

Their relative condition is therefore in many respects analagous to that of two conductors highly charged with electricities of opposite sign.

This can be verified by charging a Leyden jar from either of these conductors and testing the nature of its charge by an electrometer or a Bohnenberger's electroscope.

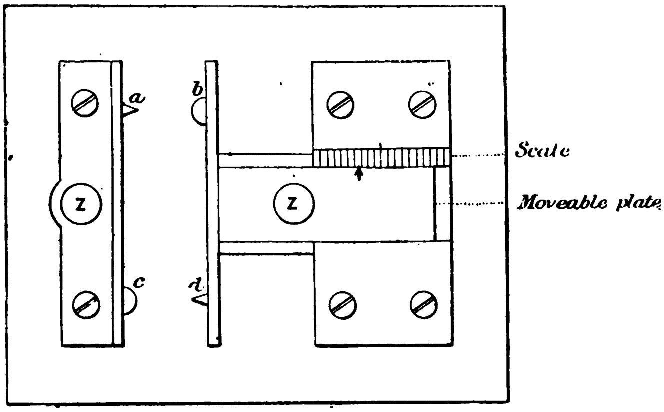
In this experiment the opposing conductors were made to consist of brass plates, each carrying a hemispherical knob and a conical point; the point on the one being opposed to the knob on the other and *vice versa*. (See sketch.)

The distance between these plates is measured by a scale and can be varied at will.

Now, if the plates be pretty close together, on setting the machine in action a continuous stream of sparks will flow across between both pair of terminals, that is, at *a b* and *c d;* but if the distance be gradually increased it will be found that at a certain distance the stream of sparks will cross between only one pair of terminals, let us say *a b*. If now the direction of the current in the primary coil, and therefore that of the induced current caused in the secondary by its interruption, be reversed, it will be found that the sparks will pass only between the terminals *c d*.

It is assumed that in each of these cases the electrical tension is relieved by the passage of the spark from the point to the knob—not from the knob to the point—*i. e.* from *a* to *b* or from *d* to *c*. Now testing the electrification of these plates by the method before referred to, it is found that in every case the plate from which the sparks pass is negatively electrified and *vice versa.*

From this it is inferred: That if two equal and similar conductors, insulated and placed in proximity, be electrified in opposite



Z. Z. Terminals

directions to an equal degree, until the tension on their opposing surfaces is relieved by the passage of a spark, the direction of the spark will be from the negative to the positive, or more generally the direction in which the electric spark tends to travel, other things being equal, is from negative to positive.

R. G. Scott,
Capt. R.E.

Chatham, 1876.

OBSERVATIONS ON A THUNDERSTORM WHICH PASSED OVER PARA ON THE 27TH DECEMBER, 1876.

By J. B. Slater.

Dear Sir,—I beg to send you an account of the following event which, should you think of sufficient interest, you will kindly cause to be communicated to the Society.

On the evening of the 27th instant a very violent storm burst over Para, in fact the most violent I have seen for several months, although this place is noted for its rainfall.

The ground-floor of the house is occupied by the offices of the Western and Brazilian Telegraph Company, and the floor above by myself and family. Upon the roof is a square room, known to us as "the Observatory." Round the roof of this room runs a gutter, from one corner of which is a pipe that carries off the rainwater to the roof of the kitchen, thence to and under a skylight, where there is a large funnel-shaped receiver, through which the water runs by a 6-inch zinc pipe to a trough or sink in the kitchen, made of the very hard wood of this country. The mouth of this pipe is about five inches from the bottom of the sink, and under it and in the sink was standing a large galvanized iron basin covering another zinc pipe leading to a drain in the earth. When this basin is full, about two inches of the pipe would be under water. At 7·20 p.m. on the above-mentioned evening, at which time the storm was about at its height, I took a small galvanized iron basin to dip out some water from the larger one which was overflowing, but at the moment I made the dip the whole place seemed to be in a blaze, and three tremendous reports were heard, while the flashes for the moment quite dazzled me.

The flashes and reports took place in the sink close by my hand (which I think was just in the water), caused, I suppose, as the current discharged itself from the upper pipe to the side of the larger basin, which by the pipe leading to the drain above referred to was in connection with earth. Immediately following the above phenomena came a tremendous peal of thunder, and all around me was a peculiar vapour and strong sulphurous smell.

After the storm, and when I had recovered from the excitement which had much frightened Mrs. Slater and an English servant, who were standing by at the time and were witnesses of the event, I found the corner tiles of the room on the roof cut clean off by the lightning in striking the rainwater pipe, along which it had run to the roof of the kitchen and funnel-shaped receiver along the pipe, until it reached the space caused by my dipping with the smaller into the larger basin.

Two large holes were fused in the latter, one very large near the top of the side, where, in my opinion, the charge was received from the pipe, the other in the bottom exactly over the pipe, which, as I stated before, leads from the sink to the earth. The edge of the smaller basin furthest from me is also fused, and from the position I held it I should judge this part was in contact with the fused part in the larger basin over the sink-pipe. But that which seems to me to be a mystery, seeing that I was neither in the least hurt, nor felt anything at the time, is the fact that the marks of my fingers where they held the basin are fused into it, the four fingers outside, and the thumb inside. How can this have happened without my feeling anything? I was standing on very dry flooring, and so may say I was well insulated. This possibly was a good thing for me.

After leaving the sink following the pipe to earth the current seems to have met with opposition at a cemented joint about ten feet down, as the pipe is there burst open; also, at its entrance into the earth, a large stone which received the pipe is broken.

During this same storm the lightning split a large mango tree not far from this.

J. Sivewright, Esq.,
Secretary Society Telegraph Engineers.

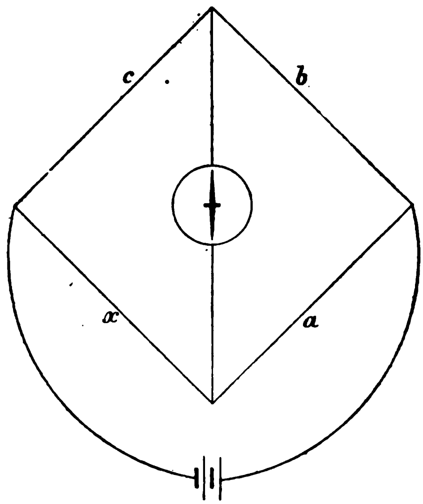
ON THE CORRECTIONS TO BE APPLIED TO THE APPARENT RESISTANCE OF THE CONDUCTOR AND INSULATOR OF A TELEGRAPH LINE WHEN DETERMINED IN THE USUAL WAY BY WHEATSTONE'S BRIDGE.

By C. Hockin, M.A.

Attention has been frequently called to the importance of properly reducing the values of the conductivity and of the insulation of a submarine cable, or of a land line, when the numerical results obtained are influenced by earth-currents, or when the line is imperfectly insulated. A complete set of formulæ have not, however,

I think been printed, and may be interesting to the readers of your journal.

To eliminate the effect of earth-currents two methods are in common use. The first and simplest (as it requires no calculation, and gives a correct result in all cases) is that which is known as "working to a false zero." Connections are arranged as desired, and the resistances of the different branches of the circuit are adjusted to what is thought to be their correct values.



The battery is then applied, and the deflection on the galvanometer observed. If on reversing the battery no change in the deflection is produced the adjustment of the bridge is correct. If otherwise, the resistances are to be altered until no change in the current through the galvanometer is observed on reversing the battery. Should it be undesirable to reverse the battery current, the deflection on the galvanometer is to be unaltered on substituting for the battery itself a circuit of the same resistance as the battery circuit, but without an electromotive force.

When the electromotive force in the circuit, the resistance of

which has to be determined, is large, but does not vary very rapidly, greater accuracy is obtainable by the second method. The apparent resistance of the circuit is observed first with the battery current in one direction and then with the current in the reverse direction.

Let x be the resistance of the circuit to be measured, in which there is an electromotive force; a, b, c, the resistances of the other branches of the circuit taken in the order indicated in the diagram, and adjusted so that there is no current through the galvanometer when the battery is applied in one direction—on reversing the battery one or more of these values have to be altered to produce a balance.

Case I.—Suppose that on reversing the battery all three quantities are altered so that a, b, c become a', b', c' respectively, and let B = resistance of battery in this case,

$$x = \frac{aa'(bc' + bc' + 2cc') + \mathrm{B}\,[a'c'(a+b) + ac(a'+b')]}{bb'(a+a') + acb' + ac'b + \mathrm{B}\,(2bb' + ab' + a'b)} \quad . \quad . \quad \mathrm{I.}$$

This formula, as it is quite general, includes every possible case. It can, however, be put into more convenient forms in special instances.

Case II.—Usually the resistances in two of the adjoining circuits of the bridge are numerically expressed as whole powers of ten. In this case putting $r = \frac{a\,c}{b}$, $r' = \frac{a'\,c'}{b'}$, the values of r and r', the apparent resistances of the cable with the two currents, can be written down without calculation, and the formula takes the more convenient form

$$x = \frac{2rr' + r'a + ra' + \mathrm{B}\left[r'\left(1 + \frac{a}{b}\right) + r\left(1 + \frac{a'}{b'}\right)\right]}{r + r' + a + a' + \mathrm{B}\left(2 + \frac{a}{b} + \frac{a'}{b'}\right)} \quad . \quad . \quad \mathrm{II.}$$

Again, it is not often necessary to alter more than one of the resistances of the circuit on reversing the battery. The resistance to be altered may be either a, b, or c.

Considering these separately,

Case III.—c only varying and corresponding (in the case of a cable) to connections in which an insulated pole of the battery is connected to the junction of the two "ratio coils" of the bridge.

The most convenient form of formula II. for this case is

$$x = \tfrac{1}{2}(r + r') - \frac{(r - r')^2}{2r + r' + 4a}\left\{1 - \frac{2\mathrm{B}\left(1 + \frac{a}{b}\right)}{r + r' + 2a + 2\mathrm{B}\left(1 + \frac{a}{b}\right)}\right\} \qquad \text{III.}$$

Case IV.—a only varying, and corresponding to connections in which an insulated pole of the galvanometer is connected to the junction between the two ratio coils. Formula II. takes the form

$$x = \frac{2rr'}{r + r'} + \frac{\mathrm{B}.c.(r - r')^2}{(r + r')\left[(r + r')(b + c) + \mathrm{B}(2c + r + r')\right]} \qquad \text{IV.}$$

Case V.—b only varying. For this case it is necessary to use for ratio coils two resistances not permanently connected as they usually are, and is interesting as being the only case in which the resistance of the battery is not always involved.

Formula I. gives the most convenient result for this case. From it we have

$$x = \frac{ac\,(b + b' + 2c)}{2bb' + c\,(b + b')}$$

$$+ \mathrm{B}.c \frac{(b - b')^2\,(c - a)}{\left\{2bb' + c\,(b + b')\right\}\left\{2bb' + c\,(b + b') + \mathrm{B}\left(b + b' + \frac{2bb'}{a}\right)\right\}}$$

$$= \frac{ac\,(b + b' + 2c)}{2bb' + c\,(b + b')} + \mathrm{B}\,\frac{c.(c - a)(b - b')^2}{[2bb' + c\,(b + b')]^2}$$

$$\times \frac{1}{1 + \mathrm{B}\,\dfrac{b + b' + \dfrac{2bb'}{a}}{2bb' + c\,(b + b')}} \quad \ldots\ldots\ldots \quad \text{V.}$$

Here b and b' are proportional to the apparent conducting powers of the circuit with the two currents, and it is clear that the second term vanishes if $a = c$.

The connections are convenient in experimental work when a very high resistance has to be determined.

Again, if the cable is compared against a fixed resistance, and the ratio of the resistance of the cable to this resistance is measured by a Varley's slide, two of the quantities a, b, c vary, but so that the sum of the two remain constant.

Case VI.—Let a and b vary, but so that

$$b = m\rho,\ c = \overline{1-m}\,.\,\rho$$
$$b' = m'\rho,\ c' = \overline{1-m'}\,.\,\rho$$

corresponding to the more usual case in which one pole of the galvanometer is attached to the sliding contact on the slide coils.

Then

$$x = a\frac{2-m-m'}{m+m'} + \frac{\mathrm{B}\left(\frac{m-m'}{m+m'}\right)^2}{1+\frac{\mathrm{B}}{\rho}+\frac{2mm'}{m+m'}\cdot\frac{\mathrm{B}}{a}} \quad . \quad . \quad . \quad . \quad \text{VI.}$$

Case VII.—If on the other hand the battery is attached to the sliding contact we must put

$$b = m\rho,\ a = \overline{1-m}\,\rho$$
$$b' = m'\rho,\ a' = \overline{1-m'}\,\rho$$

Then

$$x = c\frac{\overline{1-m}\;\overline{1-m'}\left(m+m'+\frac{2c}{\rho}\right)+\frac{\mathrm{B}}{\rho}(2-m-m')}{mm'\,(2-m-m')+\frac{c}{\rho}\,(m+m'-2mm')+\frac{\mathrm{B}}{\rho}\,(m+m')} \quad \text{VII.}$$

This formula cannot be put in a much more convenient form for calculation. The connections have the advantage that B enters into the expression divided by ρ, usually a much larger quantity than B.

If the battery resistance is inconsiderable compared with all other resistances through which the current flows, B may be neglected, and formula II. takes the simple form

$$x = \tfrac{1}{2}(r+r') - \tfrac{1}{2}(r-r')\frac{(r+a)-(r'+a')}{r+a+r'+a'} \quad . \quad . \quad . \quad \text{VIII.}$$

Which applies to all cases, and in practice is usually sufficiently accurate.

To prevent any confusion as to the meaning of the quantities a, b, c in this formula, it is sufficient to observe that in all cases b is the resistance coil of which neither end is directly connected to earth, and that if one pole of the battery is to earth c is the resistance coil, one end of which is to earth, but if one end of the galva-

nometer is to earth then a is the resistance coil with an end to earth.

In the formula just given it is assumed that one pair of observations are taken, and that the electromotive force remains constant between the observations. In practice this is not always the case. Here usually a series of observations are taken alternately with the two currents and at nearly equal intervals of time.

To reduce such a series the following method is shorter than that of reducing each pair of observations separately and equally accurate.

Take an *odd* number of observations, say n. Let $a_1\ b_1\ c_1 - a_2\ b_2\ c_2$ — &c. — $a_n\ b_n\ c_n$ be the values of $a\ b\ c$ for the first second, &c. last observation. Where $a_1\ b_3\ .\ .\ .\ a_n$, the letters with odd suffixes correspond to values obtained with one current, and $a_2\ a_4\ .\ .\ .\ a_{n-1}$ are values obtained with the other current.

Calculate $\dfrac{a_1\,c_1}{B\,(a_1+b_1)+a_1\,(b_1+c_1)}$, $\dfrac{a_2\,c_2}{B\,(a_2+b_2)+a_2(b_2+c_2)}$, &c.
for all the observations, call these numbers $A_1\ A_2\ .\ .\ .\ .\ A_n$ and also calculate $\dfrac{b_1}{B_1(a_1+b_1)+a_1\,(b_1+c_1)}$, $\dfrac{b_2}{B\,(a_2+b_2)+a_2(b_2+c_2)}$, &c.
calling the results $B_1\ B_3\ .\ .\ .\ .\ B_n$; then the most probable value of x is $\dfrac{\frac{1}{2}\,A_1+A_2+\text{\&c.}\,A_{n-1}+\frac{1}{2}\,A_n}{\frac{1}{2}\,B_1+B_2+\text{\&c.}\,B_{n-1}+\frac{1}{2}\,B_n}$ . . . . IX.

Various plans will suggest themselves to any one using this method to render the numerical work as short as may be.

If an ordinary bridge is used with coils in the ratio of 10 : 1, 100 : 1, &c., and one variable resistance, it will be most convenient to make c the variable resistance, and to keep a and b constant throughout the series.

Then $A_1\ B_1$ become $\dfrac{c_1}{c_1+b+B\left(1+\frac{b}{a}\right)}$, &c. and $\dfrac{\frac{b}{a}}{c_1+b+B\left(1+\frac{b}{a}\right)}$

respectively four references as to a book of logarithms being required to determine the pair of values. If slide resistances are used, the most convenient form of the expression is for Case VI.

$$\left.\begin{aligned} x &= a\,\frac{\frac{1}{2}\,A_1 + A_2 + \&c. + A_{n-1} + \frac{1}{2}\,A_n}{\frac{1}{2}\,B_1 + B_2 + \&c. + B_{n-1} + \frac{1}{2}\,B_n}, \\ \text{where } A_1 &= 1 - m_1 - (1 - m_1)\,\frac{\frac{B}{\rho} + m_1\frac{B}{a}}{1 + \frac{B}{\rho} + m_1\frac{B}{a}} \\ \text{and } B_1 &= \quad m_1 \quad - \quad m_1 \quad \frac{\frac{B}{\rho} + m_1\frac{B}{a}}{1 + \frac{B}{\rho} + m_1\frac{B}{a}} \end{aligned}\right\} \quad . \quad . \quad \text{X.}$$

and for Case VII.

$$\left.\begin{aligned} x &= c\,\frac{\frac{1}{2}\,A_1 + A_2 + \&c. + A_{n-1} + \frac{1}{2}\,A_n}{\frac{1}{2}\,B_1 + B_2 + \&c. + B_{n-1} + \frac{1}{2}\,B_n} \\ \text{Where } A_1 &= \frac{1 - m_1}{m_1\,\overline{1 - m_1} + \frac{c}{\rho}\,\overline{1 - m_1} + \frac{B}{\rho}} \\ \text{and } \quad B_1 &= \frac{m_1}{m_1\,\overline{1 - m_1} + \frac{c}{\rho}\,\overline{1 - m_1} + \frac{B}{\rho}} \end{aligned}\right\} \quad . \quad . \quad \text{XI.}$$

and six references to the log. book are required for each pair of values.

If the battery resistance is small, and the values of m or of c obtained with one current do not differ much from each other, it may be sufficient, the battery resistance being small, to use the mean value of m and c in the terms involving the battery resistance.

In these formulæ it is assumed that the line to be tested is absolutely insulated. In the case of submarine cables without a fault this condition nearly holds, and for short cables or for long cables in deep cold water it is very nearly true, but for cables of considerable resistance in shallow water in the tropics the correction required for imperfect insulation is considerable, and should always be taken into account.

To find this correction it is only necessary to remark that the apparent resistance determined by the bridge test is really the ratio of the electromotive force at the near end of the cable to the current entering the cable at that end. The law determining the rate of diminution of the current in the conductor, owing to the

loss by leakage through the insulator, is well known. At any point distant x from one end of the cable, the potential is given by the expression

$$A\epsilon^{\frac{x}{\lambda}} + B\epsilon^{-\frac{x}{\lambda}},$$

and the current at the same time by

$$-\frac{1}{k\lambda}\left(A\epsilon^{\frac{x}{\lambda}} - B\epsilon^{-\frac{x}{\lambda}}\right),$$

k is resistance per unit length of conductor, λ a length of cable such that the whole resistance of the conductor would equal the whole resistance of the insulator, and A B depend on the conditions at the ends of the cable.

Let R = apparent resistance of conductor of cable determined by any of the usual methods, and R' = apparent resistance of the insulator.

By determining A and B first, so that the potential vanishes at the distant end of the cable, and then, so that the current vanishes at that point, and equating the ratio of the potential to the current at the near end to R and R′ respectively in the two cases, we find that the true resistance of conductor

$$= \frac{\sqrt{RR'}}{2}\log_\epsilon \frac{\sqrt{R'} + \sqrt{R}}{\sqrt{R'} - \sqrt{R}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{XII.}$$

and true resistance of the insulator

$$= \frac{2\sqrt{RR'}}{\log_\epsilon\left(\frac{\sqrt{R} + \sqrt{R'}}{\sqrt{R} - \sqrt{R'}}\right)} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{XIII.}$$

In this form the equations are applicable to long land-lines of uniform leakage, where the insulation resistance is not large compared with the resistance of the conductor.

In the case of submarine cables it is sufficient almost always to take an approximate expression as below.

Expanding the logarithm, true resistance of conductor

$$= R\left(1 + \tfrac{1}{3}\frac{R}{R'}\right) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{XIV.}$$

true resistance of insulator

$$= R'\left(1 - \tfrac{1}{3}\frac{R}{R'}\right) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \qquad \text{XV.}$$

or more nearly true resistance of conductor

$$= R\left(1 + \tfrac{1}{3}\frac{R}{R'} + \tfrac{1}{5}\left(\frac{R}{R'}\right)^2 + \tfrac{1}{7}\left(\frac{R}{R'}\right)^3 + \&c.\right) \quad . \; . \; . \qquad \text{XVI.}$$

true resistance of insulator

$$= R'\left(1 - \tfrac{1}{3}\frac{R}{R'} - \tfrac{4}{45}\frac{R^2}{R'^2} - \tfrac{44}{945}\left.\overline{\frac{R}{R}}\right|^3 - \tfrac{428}{14175}\left(\frac{R}{R'}\right)^4 + \&\right). \text{XVII.}$$

In this form the equations are applicable to the case where the insulation resistance is apparently only two or more times that of the conductor.

On the other hand, if the apparent resistance of the insulator is only a little greater than that of the conductor, an approximate formula can be obtained in terms of $\frac{R'-R}{R'}$ which will then be a small quantity.

If $\frac{R'-R}{R}$ is a very small fraction, true resistance of conductor

$$= \tfrac{1}{2} R\left\{\log_e 4 + \log_e \frac{R'}{R'-R}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \text{XVIII.}$$

true resistance of insulator

$$= \frac{2R'}{\log_e 4 + \log_e \dfrac{R'}{R'-R}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \qquad \text{XIX.}$$

These expressions are not so much simpler than those first given as to make them of much value, and are only applicable when $\frac{R'-R}{R'}$ is so small a fraction that a like proportional error in the final result is of little consequence. It has been assumed in these formulæ that the leakage is uniform over the whole length of cable. If this is not the case, a very considerable difference results in the correction to be applied.

In many cases of submarine cables a certain length of cable is at a pretty uniform temperature, then a length in deep water at

the lowest temperature, again a length in shallow water at a higher temperature, at the further end of the cable.

In such cases the values of the resistance of the conductor and insulator, as found from determinations made at the two ends of the cable, will differ, sometimes by a quite sensible amount.

Suppose the approximate temperature of the cable to be known at different depths, so that the approximate resistances of the conductor and insulator are known also. Let r_1 r_2 r_3 be the estimated resistances of the portions of the conductor in shallow water, at near end of cable in deep water, and in shallow water again at the further end.

Also let R_1 R_2 R_3 be the estimated values of the resistance across the sheath of the insulator along the same portions of cable, and r the total resistance of the conductor of the cable, found in the ordinary way, R of the insulator.

Then true resistance of conductor

$$= r\left\{1 + \frac{r_2 + r_3}{R_1} + \frac{r_3 + r_2 r_3}{r \quad R_2} + \tfrac{1}{3}\left(\frac{r_1}{r}\frac{r_1}{R_1} + \frac{r_2}{r}\frac{r_2}{R_2} + \frac{r_3}{r} \cdot \frac{r_3}{R_3}\right)\right\} \quad \text{XX.}$$

and true resistance of insulator

$$= R\left\{1 - \left(\frac{1}{R_2} + \frac{1}{R_3}\right)\left(r_1 + r_2\frac{R}{R_3}\right) - \tfrac{1}{3}\left(\frac{r_1}{R_1} \cdot \frac{R}{R_1} + \frac{r_2 R}{R_2 R_2} + \frac{r_3}{R_3} \cdot \frac{R}{R_3}\right)\right\} \text{XXI.}$$

very nearly, assuming that the total insulation resistance of each portion of the line is much higher than the resistance of the conductor. If the resistance of the conductor thus corrected gives a mean temperature for the cable equal to that assumed in estimating r_1 r_2, &c., there is proof that these values are not far wrong. If it does not agree other values must be tried until some are found that do agree. When the values of the temperature are found pretty correctly, the insulation resistances R_1 R_2 R_3 can be more nearly estimated, and from them the total mean insulation of the cable at any standard temperature.

Formulæ VIII. and IX. are found by expanding the exponential expressions analogous to those in I. and II.

The approximate formulæ XI. XII. XV. XVI. are sufficiently accurate for all cables insulated with gutta-percha or india-rubber.

On long land-lines the leakage is much greater, and it may be necessary to use the exact expressions.

Suppose a line of uniform leakage and conductivity has been tested, and that the apparent resistance of the conductor is r_1 and of the insulator R, that then a second line is added, giving r_2 as the resistance of the conductor of the two lines together, and R_2 that of the insulators, and that on a third line being added r_3 and R_3 are the values found for these quantities for the three lines together, then the true resistance of the conductor of either one of the three lines separately is expressed in the form

$$\tfrac{1}{2}\mu \log_e \frac{1+z}{1-z} \quad . \quad . \quad . \quad . \quad . \quad \text{XXII.}$$

and the true resistance of the insulator of the same line by

$$\frac{2\mu}{\log_e \dfrac{1+z}{1-z}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{XXIII.}$$

When, for the first line tested,

$$\left.\begin{aligned} z &= \sqrt{\frac{r_1}{R_1}} \\ \mu &= \sqrt{r_1 R_1} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad \text{XXIV.}$$

for the second line

$$\left.\begin{aligned} z &= \sqrt{\frac{(r_2 - r_1)(R_1 - R_2)}{(R_2 - r_1)(R_1 - r_2)}} \\ \mu &= R_1\sqrt{\frac{(r_2 - r_1)(R_2 - r_1)}{(R_1 - r_2)(R_1 - R_2)}} \end{aligned}\right\} \quad . \quad . \quad . \quad \text{XXV.}$$

for the third line

$$\left.\begin{aligned} z &= \sqrt{\frac{(R_2 - R_3)(r_3 - r_2)}{(R_3 - r_2)(R_2 - r_3)}} \\ \mu &= R_1 \frac{R_2 - r_1}{R_1 - r_2}\sqrt{\frac{(r_3 - r_2)(R_3 - r_2)}{(R_2 - r_3)(R_2 - R_3)}} \end{aligned}\right\} \quad . \quad \text{XXVI.}$$

so that by substituting in equations XXII. and XXIII. the values of z and μ from equation XXIV. the true resistances of the conductor and of the insulators of the first section of line are found.

By substituting in equations XXII. and XXIII. the values of z and μ from equation XXV. the true values of these quantities for the second section of line are found, and by using in equations

XXI. and XXII. the values of z and μ from equations XXVI., the corresponding quantities for the third section are found.

In all these equations it is assumed that the leakage is uniform over each section of line, a condition of course only very approximately attained in practice.

Formulæ I. to XXI. contain, I think, expressions (in convenient forms for numerical calculation) for all the corrections that are to be applied to tests for the conduction and insulation of a telegraph line (supposed free from isolated faults in the case of equations XI. to XXI.)

With regard to the effect of a given uniform leakage on the signals received at the further end of the line, in the case of any land-line in which the electrostatic capacity may be neglected, it appears that if C is the current entering such a line, and C′ the current flowing out at the further end at the same time—R being the *apparent* insulation resistance of the line, and r the *apparent* resistance of the conductor—then

$$\frac{C'}{C} = \sqrt{\frac{R' - R}{R'}} \quad \ldots\ldots \quad \text{XXVII.}$$

This very simple expression gives the ratio in which signals are diminished on any line for which the values R and R′ found by ordinary tests are known.

For lines in which the total insulation resistance is many times greater than the total resistance of the conductor, it will be sufficient to take

$$\frac{C'}{C} = 1 - \tfrac{1}{2}\frac{R}{R,} \quad \ldots\ldots \quad \text{XXVIII.}$$

or more nearly

$$\frac{C'}{C} = 1 - \tfrac{1}{2}\frac{R}{R'} - \tfrac{1}{8}\left(\frac{R}{R'}\right)^2 \quad \ldots \quad \text{XXIX.}$$

expanding as usual

Clearly XXVIII is accurate within 1 per cent. for values of $\frac{R'}{R}$ not less than 5, and XXIX. for values of $\frac{R'}{R}$ not less than 3.

The following short table may be interesting as giving a general idea of the amount of current that is lost on lines which give certain test values.

Column A. contains the ratio of the apparent resistance of the line when the distant end is insulated to the apparent resistance of the line when the distant end is to earth.

Column B. contains the proportion of the current entering the line that flows out at the distant end when that end is to earth.

A	B	A	B	A	B
1·001	0·03161	1·18	0·3906	2·60	0·7845
1·002	0·04427	1·20	0·4083	2·70	0·7935
1·003	0·05469	1·22	0·4247	2·80	0·8018
1·004	0·06312	1·24	0·4399	2·90	0·8094
1·005	0·07054	1·26	0·4545	3·00	0·8165
1·006	0·07723	1·28	0·4677	3·20	0·8292
1·007	0·08338	1·30	0·4804	3·40	0·8402
1·008	0·09909	1·35	0·5091	3·60	0·8498
1·009	0·09444	1·40	0·5345	3·80	0·8584
1·010	0·09951	1·45	0·5571	4·00	0·8660
1·015	0·1216	1·50	0·5774	4·20	0·8729
1·020	0·1400	1·55	0·5957	4·40	0·8790
1·025	0·1562	1·60	0·6124	4·60	0·8846
1·030	0·1707	1·65	0·6276	4·80	0·8898
1·035	0·1839	1·70	0·6417	5·00	0·8944
1·040	0·1961	1·75	0·6547	5·20	0·8987
1·045	0·2075	1·80	0·6667	5·40	0·9027
1·050	0·2182	1·85	0·6778	5·60	0·9063
1·060	0·2379	1·90	0·6883	5·80	0·9097
1·070	0·2558	1·95	0·6980	6·00	0·9129
1·080	0·2722	2·00	0·7071	7·00	0·9258
1·090	0·2873	2·10	0·7238	8·00	0·9354
1·100	0·3015	2·20	0·7386	9·00	0·9428
1·12	0·3273	2·30	0·7518	10·00	0·9487
1·14	0·3504	2·40	0·7638	11·00	0·9535
1·16	0·3714	2·50	0·7746		

It is here supposed that the distant end of the line is in direct connection with the earth. If the current, instead of flowing directly to earth, passes through a signalling instrument of considerable resistance, the current flowing through the instrument will clearly be sensibly diminished.

If with the above notation we further put S the resistance of the instrument at the far end of the line, then the ratio of the current through the instrument to the current entering the line

$$= \frac{\sqrt{\frac{R'-R}{R'}}}{1+\frac{S}{R'}} \qquad \text{XXX.}$$

showing that the effect of the resistance of the instrument is to diminish the received current by a fractional amount nearly equal to the ratio that the resistance of the instrument bears to the apparent insulation resistance of the line, supposing the resistance of the instrument less than that of the line insulators.

ON THE MAGNITUDE OF THE SIGNALS RECEIVED THROUGH A SUBMARINE CABLE WITH VARIOUS CONNECTIONS AT EACH END, AND THE BEST RESISTANCE FOR THE RECORDING INSTRUMENT.

By C. Hockin, M.A.

Sir William Thomson has given the complete theory of the laws of the variation of the current in a submarine cable when no resistance is in circuit at either end, and a table of the numerical value of the current producing a signal at the distant end of the cable under these circumstances will be found in a paper by Prof. Jenkin on submarine cables. The object of the present paper is to determine how far these values are altered when resistances and condensers are in circuit at one or both ends of the cable.

The following is the most symmetrical solution I have found.

Assume the connections at the ends of the cable to be as indicated in the diagram.

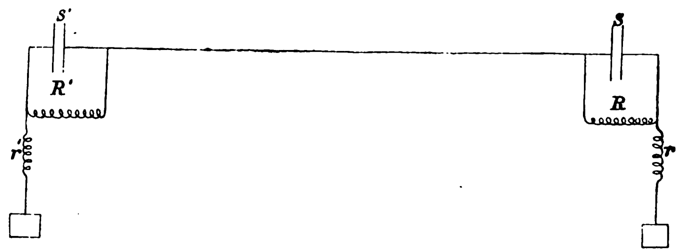


Fig. 1.

These connections will include all cases occurring in practice. For if resistances only are in circuit at either end we have only to

make R or $R' = 0$, and if condensers only are in circuit then r or $r' = 0$ and R or R′ infinite, as it may be. So if both resistances and condensers are in circuit R or R′ is infinite and r or r' finite. An outline only is given of the algebraical work, as it is rather long and not of much interest.

The following notation is used :—

x = distance of any point on the cable from *receiving* end.

v = potential at the point x and at the time t.

ζ = current at the same point and at the same time reckoned positive when flowing towards the *receiving* end.

S = capacity of condenser at receiving end.

s' = capacity of condenser at sending end.

r = resistance from condenser to earth at receiving end.

r' = resistance from condenser to earth at sending end.

R = resistance shunting condenser at receiving end.

R′ = resistance shunting condenser at sending end.

K = resistance per unit length of conductor of cable.

σ = capacity per unit length of core of cable.

l = length of cable in same units.

Also put :—

$S = \sigma l$ = total capacity of cable.

$\rho = kl$ = total resistance of cable.

$S\rho = \sigma k l^2 = a^2$.

τ = period of "dot" in seconds, or time occupied in making a single symbol.

$n = \sqrt{\frac{\pi}{\tau}}$ where $\pi = 3{\cdot}1416\ldots$ as usual.

$L = lan$, $c = \epsilon^{L}$ where ϵ is base of hyperbolic logarithms,

$$\left.\begin{aligned} a &= \frac{2an^3sr}{k},\quad \beta = \frac{an}{k}\left(\frac{r}{R}+1\right),\quad \gamma = 2n^2s,\ \delta = \frac{1}{R} \\ a_1 &= \frac{2an^3s'r'}{k},\quad \beta' = \frac{an}{k}\left(\frac{r'}{R'}+1\right),\quad \gamma' = 2n^2s',\ \delta' = \frac{1}{R'} \end{aligned}\right\} \quad . \quad 1.$$

And assume a simple harmonic variation of potential at sending end represented by

$$V_0 \sin\frac{2\pi t}{\tau} = V_0 \sin 2n^2t.$$

Then the known equation of potential is

$$\frac{d^2v}{dx^2} = a^2 \frac{dv}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 2.$$

$$\text{and } k\zeta = \frac{dv}{dx} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 3$$

The conditions at the sending end are found to be—

$$\frac{a'}{2an^3}\,\frac{d^2v}{dx\,dt} + \frac{\beta'}{an}\frac{dv}{dx} + \frac{\gamma'}{2n^2}\frac{dv}{dt} + \delta' v = \left(\frac{1}{R'} + s'\frac{d}{dt}\right) V \sin 2n^2t \quad . \quad 4.$$

and at the receiving end—

$$\frac{a}{2an^3}\,\frac{d^2v}{dx\,dt} + \frac{\beta}{an}\frac{dv}{dx} - \frac{\gamma}{2n^2}\frac{dv}{dt} - \delta v = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 5.$$

The solution given by Fourier suited to the case in question is—

$$v = \epsilon^{anx} \left((A \sin (2n^2t + anx) + B \cos (2n^2t + anx)\right)$$
$$+ \epsilon^{-anx} (A' \sin (2n^2t - anx) + B' \cos (2n^2t - anx) \quad . \quad . \quad . \quad 6.$$

A B, A′ B′ have to be determined so as to satisfy equations 4 and 5 identically. Substituting for v in 4 and 5 its value from 6, and equating to O, the coefficients of sin T and cos T, A B A′ B′ are determined. Then these values substituted in the value of $\frac{1}{\kappa}\frac{dv}{dx}$ from equation 3, give the value of the current at the point x, and if x is made equal to O the current at the receiving end called Γ.

$$\text{Put} \quad \left.\begin{aligned} e &= -a + \beta - \delta, & e' &= -a' + \beta' - \delta \\ f &= -a - \beta + \gamma, & f' &= -a' - \beta' + \gamma \\ g &= a - \beta - \delta, & g' &= a' - \beta' - \delta' \\ h &= a + \beta + \gamma, & h' &= a' + \beta' + \gamma' \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad 7.$$

$$\left.\begin{aligned} \lambda &= \frac{eh - fg}{e^2 + f^2} & \lambda' &= \frac{e'h' - f'g'}{e'^2 + f'^2} \\ \mu &= \frac{fh + eg}{e^2 + f^2} & \mu &= \frac{f'h' + e'g'}{e'^2 + f'^2} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad 8$$

Then we shall find

$$\Gamma = \frac{an}{k\sqrt{2}} \; \frac{\sqrt{\{\lambda^2 + (1+\mu)^2\}\,\{\lambda'^2 + (1+\mu')^2\}}}{\sqrt{G}} \sin (2n^2t + \Pi) \quad . \quad 9.$$

Where

$$G = (\lambda^2 + \mu^2)\ (\lambda'^2 + \mu'^2)\ c^2 + \frac{1}{c^2} - 2\ (\lambda\mu' + \lambda'\mu)\ \sin 2L \left.\begin{matrix} \\ \\ \end{matrix}\right\} \quad 10^*$$
$$+ 2\ (\lambda\lambda' - \mu\mu')\ \cos 2L$$

and Π. is a complex function of $\lambda\ \mu$, $\lambda'\ \mu'$, &c. which I do not give as it is not required for the purposes of this paper.* Equations 9 and 10 are exact, and enable us to find the maximum value the current reaches at the end of a submarine cable under the conditions stated above.

In the practical case some simplifications can be introduced.

First. The electromotive force of the battery does not vary continuously like the function sin T, but has a given constant value for a certain time, and then suddenly alters to nothing, or to an equal negative value.

Secondly. The cable being worked at the rate at which long cables are ordinarily worked, the value of c is very large, so that the first term in G is much larger than the remaining terms, and for our purpose it will be sufficient to consider the numerical value of this term only.

When the second condition just given holds, the first condition increases the value of the received current in the proportion of $4 : \pi$ nearly, and we have to a sufficient degree of accuracy in practice—

$$\Gamma = \frac{an}{ck\sqrt{2}}\sqrt{\frac{\{\lambda^2 + (1+\mu)^2\}\ \{\lambda'^2 + (1+\mu')^2\}}{(\lambda^2+\mu^2)\ (\lambda'^2+\mu'^2)}} \quad . \quad . \quad 11.$$

From equations 9 and 10, where

$$\lambda = \frac{-2\gamma\ (a-\beta) - 2\delta\ (a+\beta)}{\gamma^2 + \delta^2 + 2\ (a^2+\beta^2) + 2\delta\ (a-\beta) - 2\gamma\ (a+\beta)} \quad . \quad 12.$$

$$\mu = \frac{\gamma^2 + \delta^2 - 2\ (a^2+\beta^2)}{\gamma^2 + \delta^2 + 2\ (a^2+\beta^2) + 2\delta\ (a-\beta) - 2\gamma\ (a+\beta)} \quad . \quad 13.$$

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* The general expression is—

$$-\frac{KrG}{an} = c\left(A_1 \cos(T-L) + A_2 \sin(T-L)\right) - \frac{1}{c}(B_1 \cos(T+L) + B_2 \sin(T+L)).$$

Where

$$2A_1 = (\lambda^2+\mu^2+\lambda+\mu)\ (\lambda'^2+\mu'^2+\lambda'+\mu') - 2\lambda\lambda'$$
$$2A_2 = (\lambda^2+\mu^2-\lambda+\mu)\ (\lambda'^2+\mu'^2-\mu'+\mu') - 2\lambda\lambda'$$
$$2B_1 = (-\lambda+\mu+1)\ (-\lambda'+\mu'+1) - 2\lambda\lambda'$$
$$2B_2 = (\lambda+\mu+1)\ (\lambda'+\mu'+1) - 2\lambda\lambda'$$

λ' and μ' are the same functions of α', β', γ', δ' as λ and μ are of $\alpha, \beta, \gamma, \delta$, and α and β . . . are defined by equations (1).

All resistances and capacities are here expressed in absolute measure. It will only be necessary to remember that one ohm is 10^7 in metre-seconds, one microfarad is 10^{-13} in metre-seconds.

We will now put equation (17) in a convenient form for the different cases occurring in practice.

Cases I. and II.

No condensers used.

Here there is a resistance at the sending end, viz. the resistance of the battery, and at the receiving end the resistance of the recording, instrument.

We therefore put $R = R' = s = s' = 0$ and r and r' finite.

$$\therefore \beta = \infty, \ \delta = \infty, \text{ while } \frac{\beta}{\delta} = \frac{a n r}{k}.$$

If we put
$$\frac{a n r}{k} = p$$

then
$$\lambda = \frac{-2p}{2p^2 + 1 - 2p}, \ \mu = \frac{1 - 2p^2}{2p^2 + 1 - 2p}.$$

And after reductions

$$\Gamma = V \frac{2\sqrt{2}\, an}{ck} \ \frac{1}{\sqrt{p^2 + (p+1)^2}} \times \frac{1}{\sqrt{p'^2 + (p'+1)^2}} \quad . \quad 14.$$

Could both the resistances be neglected we should have

$$\Gamma = V \frac{2\sqrt{2}\, an}{ck} \quad . \ . \ . \ . \ . \ . \ . \quad 15.$$

This is the expression tabulated in Professor Jenkin's paper, in which an arbitrary unit of time is adopted, originally introduced by Sir William Thomson for convenience in calculating the value of the series expressing the law followed by the current arriving at the distant end of a cable.

Cases III. and IV.

Condenser at one end only.

Let condenser be at receiving end. There will, as before, be two resistances—the battery and the galvanometer.

Then r' r and s are finite, $R' = s' = 0$ and $R = \infty$.

If we put

$$p = 2\,n^2 sr,\ q = \frac{2\,nsk}{a},\ p' = \frac{a\,n\,r'}{k} \quad \dots\dots\dots \quad 16.$$

we shall find

$$\left.\begin{aligned} \lambda &= \frac{-2q\,(p-1)}{q^2+2\,(p^2+1)-2q\,(p+1)} \\ \mu &= \frac{q^2-2\,(p^2+1)}{q^2+2\,(p^2+1)-2q\,(p+1)} \end{aligned}\right\} \quad \dots\dots \quad 17.$$

And after a rather long reduction

$$\Gamma = \frac{2\sqrt{2}an}{ck} \times \frac{\mathrm{V}\,q}{\sqrt{2p^2+2pq+1+(q+1)^2} \times \sqrt{p'^2+(1+p')^2}} \quad 18.$$

Where it is to be noted that, if the signalling instrument is at the same end as the condenser,

r = resistance of instrument,

r' = resistance of battery. Case III.

But if the battery and condenser are at one end and the instrument at the other end of the cable

r' = resistance of instrument,

r = resistance of battery. Case IV.

CASE V.

Condensers at both ends of the line.

Let r and r' be the resistances of the battery and instrument respectively.

$\mathrm{R} = \mathrm{R}' = \infty$ and $r\ r'\ s\ s'$ are finite.

$$\left.\begin{aligned} \text{Put } p &= 2n^2 sr,\quad q = \frac{2nsk}{a} \\ p' &= 2n^2 s'r',\ q' = \frac{2ns'k}{a} \end{aligned}\right\} \quad \dots\dots \quad 19.$$

then

$$\left.\begin{aligned} \lambda &= \frac{-2q\,(p-1)}{q^2+2\,(p^2+1)-2q\,(1+p)} \\ \mu &= \frac{q^2-2\,(p^2+1)}{q^2+2\,(p^2+1)-2q\,(1+p)} \end{aligned}\right\} \quad \dots\dots \quad 20.$$

$\lambda'\ \mu'$ are the same functions of p' and q' that $\lambda\ \mu$ are of p and q, and ultimately we should find

$$\Gamma = \frac{2\sqrt{2}an}{ck}\,\frac{\mathrm{V}\,qq'}{\sqrt{2p^2+2pq+1+(1+q)^2}\,\sqrt{2p'^2+2p'q'+1+(1+q')^2}} \quad 21$$

Equations 14 to 21 give the value of the maximum current received at the end of the line when a simple harmonic variation of potential occurs at sending end.

If the battery has a constant electromotive force, *the size of the signal produced by contacts battery, earth battery, earth,* &c. is found by multiplying these expressions by $4:\pi$, or 1·273 nearly.

To illustrate these formulæ we take a numerical example:—

Consider a cable 2,500 knots in length with a resistance of 3 ohms per knot and a capacity of 0·4 microfarads per knot,

then $$L = anl = \sqrt{\frac{\kappa\sigma l^2\pi}{\tau}} = \sqrt{\frac{23\cdot 6}{\tau}}.$$

Further, let the rate of signalling be N words per minute,

then $$\tau = \frac{60}{23N}, \quad \text{and} \quad L = 2\cdot 88\sqrt{N}.$$

If $N = 10$, by no means a great speed for such a cable, c or ϵ^L becomes large, and our approximations for Γ are seen to be practically sufficient.

Case I.

Let a galvanometer of 3,000 units be in circuit at one end of the line and a battery of 75 units at the other, then in equation 14

$$p = \frac{anr}{\kappa} = \frac{anl}{\kappa l}r = L\frac{r}{\rho} = 2\cdot 88\sqrt{N}\frac{r}{\rho} = 3\cdot 64 \text{ nearly.}$$

And also $$\frac{1}{\sqrt{p^2 + (1+p)^2}} = \frac{1}{5\cdot 9}.$$

Or the resistance of the galvanometer reduces the signals nearly six times.

To find the effect of the battery put $r = 75$ ohms,

$$p' = 2\cdot 88\sqrt{10}\,\frac{r'}{\rho} = 0\cdot 091.$$

And $$\frac{1}{\sqrt{p'^2 + (1+p')^2}} = 0\cdot 91.$$

Or the resistance of the battery, which is only 1 per cent. of the resistance of the cable, reduces the signals 9 per cent. and the two resistances together reduce the current about $6\frac{1}{2}$ times.

Next, suppose the galvanometer to have a resistance of 500 ohms,

p will become 0·607 and $\frac{1}{\sqrt{p^2+(1+p)^2}} = 0{\cdot}582$ nearly. The current is therefore reduced to nearly 0·6 of its maximum value.

The relative sensitiveness of the two instruments is as $\sqrt{3000}$: $\sqrt{500}$ or as 2·45 : 1, and the size of the signals received on the two instruments as 2·45 : 5·9 × 0·582, or as 0·713 : 1, showing that other things being the same the galvanometer of 500 ohms resistance gives larger signals than one of 3,000 ohms in the proportion of 10 : 7 nearly.

To determine the *best resistance* for the galvanometer we must make

$$\frac{\sqrt{r}}{\sqrt{p^2+(1+p)^2}} \text{ a maximum,}$$

or since p varies as r

$$\frac{p}{p^2+(1+p)^2} \text{ a maximum,}$$

$$\text{or } \; 2p+\frac{1}{p}+2, \text{ a minimum,}$$

$$\text{or } \; p=\frac{\sqrt{2}}{2}=0{\cdot}707.$$

So that

$$\frac{anr}{k}=\frac{1}{\sqrt{2}}, \text{ or } r\sqrt{\frac{k\sigma\pi}{\tau k^2}}=\frac{1}{\sqrt{2}}=r\sqrt{\frac{k\sigma l^2}{k^2l^2}\cdot\frac{\pi}{\tau}}=\frac{r}{\rho}\sqrt{\frac{\pi}{\tau}\rho \mathrm{S}}.$$

$$\text{Whence } r=\frac{\rho}{\sqrt{2}}\sqrt{\frac{\tau}{\pi}\cdot\frac{1}{\rho\mathrm{S}}}=\frac{0{\cdot}64\,\rho}{\sqrt{\mathrm{N}\rho\mathrm{S}}} \quad \ldots\ldots\ldots \quad 22$$

The rule in words is therefore—Fix on the number of words per minute likely to be usually sent on the line.

Multiply the number of words per minute by the total capacity of the cable expressed in microfarads, and the product by the total resistance of the conductor in *megohms*.

Extract the square root of the triple product. The resistance of the galvanometer should be 0·64 times the resistance of the cable divided by the square root already found.

From this it appears that for a cable of given working speed the resistance of the galvanometer should be proportional to the resistance of the conductor; thus the best galvanometer for a line

with a core 400 lbs. copper per knot has a much lower resistance than one best for a core of 107 lbs. per knot, the theoretical speed of the two lines being the same.

Cases III. and IV.

These are cases more generally realised on a long submarine cable where earth currents necessitate the use of a condenser at one end of the line at least.

The formula shows at once that the current at the distant end of the cable only approaches to the current received when no condenser is in circuit as the capacity of the condenser approaches infinity.

Neglecting resistances in the first instance the condenser diminishes the current in the ratio of

$$\frac{q}{\sqrt{1+(1+q)^2}} : 1. \quad \text{Where } q = \frac{2nsk}{a} = 2\sqrt{\frac{\pi}{\tau\sigma k l^2}} \times skl$$

$$= 2\sqrt{\frac{\pi}{\tau S \rho}} \times s\rho$$

$$= 2\sqrt{\frac{3{\cdot}14 \times \mathrm{N} \times 23}{60\,\mathrm{S}\rho}} \times s\rho$$

Taking the numerical case before used

$$q = 2\sqrt{\frac{3{\cdot}14 \times 230}{60 \times 7{\cdot}5}} \times s \times \rho = 2{\cdot}53\, s\rho$$

first, if $s = 100$ microfarads, or 10^{-11},

$$s\rho = 10^{-11} \times 7{\cdot}5 \times 10^{10} = 0{\cdot}75,$$

and $q = 1{\cdot}9$ nearly,

then $$q\sqrt{\frac{1+(q-1)^2}{4+q^4}} = 1{\cdot}9\sqrt{\frac{1{\cdot}81}{4+13{\cdot}03}} = \frac{1}{1{\cdot}61} \text{ nearly,}$$

or the signals are diminished 1·61 times.

If we suppose the condenser to have 210 microfarads capacity, q will nearly equal 4, and the ratio $4\sqrt{\frac{10}{256}} = \frac{4}{5}$ nearly.

It appears then that if the resistance of the battery and also of the receiving instrument is very small, a capacity of 210 microfarads gives four-fifths of the maximum current that can be received

through the cable by any connections, and that a capacity of 100 microfarads gives a signal five-eighths of the maximum.

When the signals are read on a galvanometer the resistance cannot be neglected, and the important point is to know whether the signal is largest if the galvanometer and condenser are at the same or at opposite ends of the cable.

Taking the same instance as before, let $2=3{,}000$ ohms, then

$$p=2n^2sr=\frac{2\pi}{\tau}100\times 10^{-13}\times 3{,}000\times 10^7=0{\cdot}6\frac{\pi}{\tau}=7{\cdot}23.$$

The expression for the diminution of the received current is

$$\frac{q}{\sqrt{2p^2+2pq+1+(1+q)^2}}=\frac{1}{6{\cdot}37},$$

showing that a resistance of 3,000 ohms inserted at the same end of the cable as the condenser reduces the signals from $\frac{1}{1{\cdot}61}$ to $\frac{1}{6{\cdot}37}$, or $3{\cdot}95$ times.

Inserted at the other end we have seen that it would have reduced the signals $5{\cdot}9$ times. Therefore the *most sensitive arrangement* is to send *directly into the cable* and *to receive with a condenser in circuit.*

To take the second instance in which the galvanometer has 500 ohms resistance, *p becomes* $1{\cdot}2$ nearly, and the ratio

$$=1{\cdot}9\sqrt{\frac{0{\cdot}13}{2{\cdot}15}}=\frac{1}{2{\cdot}14}\text{ nearly;}$$

the resistance of the galvanometer, 500 ohms, has therefore reduced the signals from $\frac{1}{1{\cdot}61}$ to $\frac{1}{2{\cdot}14}$ or $1{\cdot}33$ times. Inserted at the other end of the line we have seen that it would have reduced the current $1{\cdot}72$ times. The size of the signals on the two galvanometers will be as $\sqrt{\frac{3000}{500}}\times\frac{2{\cdot}14}{6{\cdot}37}$, or about 18 % larger on the galvanometer of least resistance.

The best value of r is found by making

$$\frac{\sqrt{r}}{\sqrt{2p^2+2pq+(q+1)^2+1}}\text{ a maximum.}$$

Or, since p is proportional to r,

$$2p+2q+\frac{1+(q+1)^2}{p} \text{ a minimum.}$$

Whence
$$p=\sqrt{\frac{1+(q+1)^2}{2}}$$

or
$$r=\frac{\tau}{2\sqrt{2\pi s}}\sqrt{1+\left(1+2\,s\,\rho\sqrt{\frac{\pi}{\tau\,S\,\rho}}\right)^2}$$

$$=\frac{0{\cdot}293}{N\,s}\sqrt{1+\left(1+2{\cdot}19\,\frac{s\rho\sqrt{N}}{\sqrt{S\rho}}\right)^2}$$

Or, if $\rho\,s$ and S are expressed in ohms and microfarads respectively, the resistance of the galvanometer should be

$$\frac{293000}{Ns'}\sqrt{1+\left(1+\frac{2{\cdot}19}{1000}\,\frac{s'\rho'\sqrt{N}}{\sqrt{S'\rho'}}\right)^2}=r' \quad . \quad . \quad 24$$

Where N = number of words sent per minute,
s' = capacity of the condenser in *microfarads*,
ρ' = total resistance of conductor of cable in *ohms*,
S′ = total capacity of cable in microfarads,
r' = resistance of galvanometer in ohms,

suppose, as before,

$$N = 10,\ s' = 100,\ S' = 1{,}000,\ \rho = 7{,}500.$$

$$r' = 293\sqrt{1+\left(1+\frac{2{\cdot}19}{1000}\,\frac{750000\times\sqrt{10}}{\sqrt{7500000}}\right)^2},$$

$$= 293\ \sqrt{1+(2{\cdot}9)^2} = 293\times\sqrt{9{\cdot}4} = 900 \text{ ohms nearly.}$$

As a second instance let the cable have the same "working speed," but let the core have a resistance of 12 ohms per knot instead of 3, and let the capacity be 0·3 microfarads per knot.

Then, as before, $S'\rho' = 7{,}500{,}000$, while ρ' becomes 17,320 nearly,

and
$$r' = 293\cdot\sqrt{1+\left(\frac{2{\cdot}19}{1000}\,\frac{1732000\sqrt{10}}{\sqrt{7500000}}+1\right)^2},$$

$$= 293\ \sqrt{1+(1+43{\cdot}8)^2} = 1600 \text{ ohms nearly.}$$

Further, if the galvanometer had its best resistance, the size of the signals would be proportional to

$$\frac{q\sqrt{r}}{\sqrt{2p^2 + 2pq + 1 + (q+1)^2}}$$

or

$$\frac{q\sqrt{\frac{1}{2n^2s}}\sqrt{\frac{1+(1+q)^2}{2}}}{\sqrt{2+2(1+q)^2 + \sqrt{2}\,q\sqrt{1+(1+q)^2}}}$$

If the direct method of signalling had been employed, the largest signals obtainable would have been proportional to

$$\frac{\sqrt{\frac{\kappa\sqrt{2}}{2an}}}{\sqrt{\frac{1}{2}+\left(1+\frac{1}{\sqrt{2}}\right)^2}} = \sqrt{\frac{\kappa}{2an}(\sqrt{2}-1)}.$$

The last expression is always greater than the former, or the *method of signalling with condensers is less sensitive than that of signalling* directly in all cases.

Case V. Here a condenser at both ends of the line is always in circuit. This method has some advantages over all others, and it will be worth while to examine the numerical values more closely.

First, it is evident that the battery resistance should be as small as possible and should not exceed one per cent. of the resistance of any cable of considerable resistance.

Secondly, the total capacity of condensers available should be equally divided between the two ends of the cable.

Then, if the battery resistance is not larger than that stated above, we have size of signals received on a galvanometer of resistance r proportional to

$$\frac{q q' \sqrt{r}}{\sqrt{1+(1+q')^2}\,\sqrt{2p^2+2pq+1+(1+q)^2}} = \text{M say.}$$

Or, if $q = q'$,

$$\text{M} = \frac{q^2\sqrt{r}}{\sqrt{1+(1+q)^2}\,\sqrt{2p^2+2pq+1+(1+q)^2}}.$$

As before M is a maximum when

$$p = \sqrt{\frac{1+(1+q)^2}{2}},$$

and in that case

$$M = \frac{q^2}{\sqrt{2n^2 s}\sqrt[4]{2}} \times \frac{1}{\sqrt[4]{1+(1+q)^2}} \times \frac{1}{\sqrt{2(1+(1+q)^2)+q\sqrt{2}\sqrt{1+(1+q)^2}}}$$

$$= \frac{q^2}{2n\sqrt{s}} \times \frac{1}{\sqrt{1+(1+q)^2}} \times \frac{1}{\sqrt{q+\sqrt{2}\sqrt{1+(1+q)^2}}}$$

Using the same notation as before,

$$q = 2{\cdot}19\,\frac{s\rho\sqrt{N}}{\sqrt{S\rho}},$$

and

$$2n\sqrt{s} = 2{\cdot}19\sqrt{Ns},$$

and

$$M = \frac{2{\cdot}19\,\rho s\sqrt{s}\sqrt{N}}{\sqrt{1+\left(1+2{\cdot}19\frac{s\rho\sqrt{N}}{\sqrt{S\rho}}\right)^2}}$$

$$\times \frac{1}{\sqrt{2{\cdot}19\frac{s\rho\sqrt{N}}{\sqrt{S\rho}}+\sqrt{2}\sqrt{1+\left(1+2{\cdot}19\frac{s\rho\sqrt{N}}{\sqrt{S\rho}}\right)^2}}}.$$

The minimum signal produced by a "dot" contact will be equal to that produced by the battery acting directly through the galvanometer, and such an external resistance as will make the total resistance of the circuit = Q.

Where

$$Q = \frac{\kappa\sqrt{2}}{16\,\pi\,an}\,\epsilon^{anl}\,\frac{1}{q^2}\sqrt{\{2p^2+2pq+1+(1+q)^2\}\{1+(1+q)^2\}}$$

Or, if

$$p = \sqrt{\frac{1+(1+q)^2}{2}},$$

$$Q = \frac{\kappa\sqrt{2}}{16\,\pi\,an}\,\epsilon^{anl}\,\frac{1}{q^2}\{1+(1+q)^2\}\sqrt{2+\frac{q\sqrt{2}}{\sqrt{1+(1+q)^2}}},$$

Or, if further

$$\frac{1+(1+q)^2}{q^2} = z^2,$$

$$Q = \frac{1}{16} \times \frac{\kappa\sqrt{2}}{\pi an}\epsilon^{anl}\,z^2\sqrt{2+\frac{\sqrt{2}}{z}}.$$

Or, adopting the same notation as before for ohms and microfarads,

$$Q = E'\epsilon^{L'}z^2\sqrt{2+\frac{\sqrt{2}}{z}} \text{ in ohms.}$$

Where $$z' = \sqrt{\frac{1+\left(1+\frac{2\cdot 19}{1000}\times\frac{s'\rho'\sqrt{N}}{\sqrt{S'\rho'}}\right)^2}{\frac{4\cdot 8}{1000000}\times\frac{s'^2.\rho'.N}{S'}}} \text{ nearly;}$$

$$L' = \frac{1\cdot 095}{1000}\sqrt{NS'\rho'} \text{ nearly;}$$

$$E' = 25\cdot 6\sqrt{\frac{\rho'}{S'N}} \text{ nearly.}$$

$$\log_{10} Q = \log_{10} E' + 0\cdot 4343\, L + 2\log z' + \tfrac{1}{2}\log\left(2+\frac{\sqrt{2}}{z'}\right).$$

The deflection produced by the battery to be used in signalling through the resistance Q will be the size of the *smallest* signal produced when working at the rate of N words per minute.

We will take ten cases using round numbers not differing much from the figures occurring in the case of some existing cables.

Length in knots.	Capacity per knot in microfarads.	Resistance per knot in ohms.	Working speed assumed in words per minute.
2500	0·40	3·0	12·0
1800	0·40	3·0	25·0
2000	0·36	4·0	20·0
1800	0·34	6·5	15·0
1500	0·35	10·0	14·0
900	0·30	10·0	30·0
850	0·30	12·0	15·0
1800	0·30	10·0	10·0
1600	0·30	11·6	12·0
925	0·30	8·2	30·0

and suppose that 100 microfarads are used in all cases at each end of the cable, and the galvanometer of best resistance.

Then for these cases we shall have the best resistance of the galvanometer = r' and the constant of the instrument = Q' where Q' and r' are as follows:—

Example.	Value of r' in ohms.	Value of Q' in ohms.
I.	790	520,000
II.	483	1,000,000
III.	640	10,000,000
IV.	942	340,000
V.	626	460,000
VI.	780	400,000
VII.	1245	69,000
VIII.	1465	290,000
IX.	1418	490,000
X.	717	340,000

This table gives the approximate value of the "constant" of the galvanometer used for signalling on the different lines considered, the deflection produced by the smallest signal (like the last dot of the figure 5) being taken as the unit deflection, and also the best value for the resistance of the galvanometer. It will be seen that the resistance of the galvanometer is generally less than that usually employed.

On a *given cable* the resistance of the galvanometer should be less, as the working speed is greater. It will therefore be best to adjust the resistance of the galvanometer for the average speed of working rather than for the maximum speed, and to have the galvanometer coil divided into two or three parts, with separate terminals.

The preceding formula enables us to estimate the size of the minimum signal whatever connections are used and whatever resistances are in circuit at either end of the line.

It is also clear from these figures that every care should be taken in the manufacture of "speaking" galvanometers; the magnets made of carefully selected steel, the mirror as light as possible, and the galvanometer coil of the proper form.

Hitherto the size only of the signals has been considered. The general formula 9 gives an expression by which the shape of the signal, produced by suddenly raising the potential at A, or putting on the battery, can be calculated. If we write that equation in the form $\gamma = f(\tau)$, then for a sudden finite increase of potential we shall have

$$\frac{\pi}{4}\gamma = f(\tau) + \tfrac{1}{3} f(3\tau) + \tfrac{1}{5} f(5\tau) + \&c.$$

and by making τ large compared with $k\,c\,l^2$ the approximation may be carried so far as desired, but the series converges very slowly when $\frac{\tau}{k\,c\,l^2}$ is greater than unity, and the process would be very tedious. I do not know how to put the solution in a convenient form for numerical calculation.

However, in one very important case, an approximation can be obtained as follows:—If condensers are employed both for sending and for receiving and of capacity small compared with that of the cable, it is clear that the current will not be very much altered by a small internal resistance in the condenser. In fact, instead of a condenser we may suppose a short length of cable to be used for sending and receiving. This case solved leads to a series converging rapidly.

Imagine a cable of infinite length extending in both directions from its centre O, divide it into lengths O A, A B, B C, C D, &c., in one direction, and into equal divisions O A′, A′B′, B′C′ in the opposite direction, and let

$$A\,A' = B\,C = D\,E = \&c. = B'C' = D'E' = \&c.;$$

and let

$$A\,B = C\,D = E\,F = \&c. = A'B' = C'D' = \&c.$$

Let $2\,\lambda$ be the length of each of the first series of segments, and $2\,l$ that of the second series.

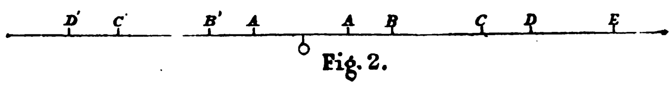

Fig. 2.

Let the first series of sections be charged to the potential $V\,\frac{l}{l+\lambda}$ and the second to the potential $-\,V\,\frac{\lambda}{l+\lambda}$.

There will be as much positive electricity as negative electricity

in the cable, and from the symmetry it is evident that no current will flow across the middle points of either of the segments, and the distribution of electricity and current from the middle point of any segment to the middle point of the adjoining segment will be the same as if this portion were detached from the cable, the ends insulated, and the portion of the cable charged as first described, viz., a length δ to potential $V \frac{l}{l+\lambda}$ and the remainder to potential $-V \frac{\lambda}{l+\lambda}$.

The current will moreover be the same as if the outside of the cable for a length λ were lowered to potential $-V$, and the remaining length unaltered initially.

This is Thomson's method of electrical images. Applying Poisson's expression for discontinuous functions, the original distribution of potential in the infinite cable is represented by

$$v = \frac{1}{\pi} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \phi(v) \cos w (v-x) \, dv \, dw \quad . \quad . \quad . \quad . \quad . \quad . \quad 1$$

and the distribution of potential at time t by

$$v = \frac{1}{\pi} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \phi(v) \, . \cos w (v-x) \, . \, \epsilon^{\frac{-w^2 t}{a}} \, . \, dv \, dw \quad . \quad . \quad . \quad . \quad 2.$$

Where $\phi(v)$ is to be made equal to $V \frac{l}{l+\lambda}$ when v lies between $\pm (2n\,l + \overline{2n+1}\,\lambda)$ and $\pm (\overline{2\,n\,l} + \overline{2\,n - 1}\lambda)$ and to $-V \frac{\lambda}{l+\lambda}$ when v lies between $\pm (2n\,l + \overline{2\,n - 1}\,\lambda)$ and $\pm \overline{2\,n - 1}\,l + \overline{2n - 1}\lambda)$ for all integral values of n, o to ∞.

Integrating equation 2 with regard to w we find

$$v = V \sqrt{\frac{a}{\pi t}} \int_{-\infty}^{\infty} \phi(v) \, . \, \epsilon^{-\frac{(v-x)^2}{4t} . a} . \, dv \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 3.$$

$$= V \sqrt{\frac{a}{\pi t}} \sum_{n=\infty}^{n=1} \left[\left\{ \left(\int_{\beta_1}^{\alpha_1} + \int_{\beta_2}^{\alpha_2} + \int_{\beta_3}^{\alpha_3} \right) \frac{l}{l+\lambda} - \left(\int_{\beta_4}^{\alpha_4} + \int_{\beta_5}^{\alpha_5} \right) \frac{\lambda}{l+\lambda} \right\} \epsilon^{-\frac{(v-x)^2}{4t} a} \, dv \right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 4.$$

Where

$$\left.\begin{array}{ll}
a_1 = 2nl + (2n+1)\lambda & \beta_1 = 2nl + (2n-1)\lambda \\
a_2 = -2nl - (2n-1)\lambda & \beta_2 = -2nl - (2n+1)\lambda \\
a_3 = \lambda & \beta_3 = -\lambda \\
a_4 = 2nl + (2n-1)\lambda & \beta_4 = 2(n-1)l + (2n-1)\lambda \\
a_5 = -2(n-1)l - (2n-1)\lambda & \beta_5 = -2nl - (2n-1)\lambda
\end{array}\right\} \quad 5.$$

the result can be readily expressed in terms of Euler's first integral, but is not of much interest.

For the current at any point of the cable, if we write

$$v = \Sigma\,(\mathrm{A} + \mathrm{B} + \&\text{c.}) \times \mathrm{V}\sqrt{\frac{a}{\pi t}},$$

then $$-\kappa\gamma = \frac{\delta v}{\delta x} = \Sigma\left(\frac{\delta \mathrm{A}}{\delta \mathrm{B}} + \frac{\delta \mathrm{B}}{\delta x} + \&\text{c.}\right) \times \mathrm{V}\sqrt{\frac{a}{\pi t}}.$$

When $\frac{\delta \mathrm{A}}{\delta x}$ is of the form $\frac{\delta}{\delta x}\int_\beta^a \epsilon^{-\frac{(v-x)^2}{4t}\cdot a} \cdot dv$

$$= \epsilon^{-\frac{(\beta-x)^2}{4t}a} - \epsilon^{-\frac{(a-x)^2}{4t}\cdot a}.$$

$$= \psi(\beta - x) - \psi(a - x)$$

suppose; then

$$\begin{aligned}\gamma = \mathrm{V}\frac{1}{k}\sqrt{\frac{a}{\pi t}}\Big\{\Sigma_{n=\infty}^{n-1}\Big(&\psi(a_1-x) - \psi(\beta_1-x) + \psi(a_2-x) - \psi(\beta_2-x) \\
&+ \psi(\lambda-x) - \psi(-\lambda-x)\Big)\frac{l}{l+\lambda} - \Sigma_{n=\infty}^{n-1}\Big(\psi(a_4-x) - \psi(\beta_4-x) \\
&+ \psi(a_5-x) - \psi(\beta_5-x)\Big)\frac{\lambda}{l+\lambda}\Big\} \quad \ldots\ldots\ldots\ldots \quad 6.\end{aligned}$$

where $a\ \beta$ have the values already given.

If we suppose that the capacity of the sending and receiving condensers is the same we must put $x = \lambda$ for the current at the sending end of the cable, and $x = l$ for the current at the receiving end of the cable.

Substituting these values, collecting the terms and remembering that $\psi(u) = \psi(-u)$ for all values of u, we have ultimately for sending end, or current entering the cable,

$$\gamma = \mathrm{V}\frac{1}{k}\sqrt{\frac{a}{\pi t}}\left\{1 - \Sigma_{n=\infty}^{n=1}\Big(\psi(2nl + 2(n-1)\lambda) + \psi\big(2(n-1)l + 2n\lambda\big) - 2\psi(2nl + 2n\lambda)\Big)\right\} \quad 7.$$

those of the traced curve on any abscissa are the ordinates of the signal. The curve corresponding to these ordinates having been drawn, the effect of a second signal is found by superposing the curve for the second signal or that of the first. In this way the curves

Value of $\frac{a\,l^2}{t}$	Value of γ	Value of $\frac{a\,l^2}{t}$	Value of γ	Value of $\frac{a\,l^2}{t}$	Value of γ	Value of $\frac{a\,l^2}{t}$	Value of γ
50	0·03162	14·8	9·009	8·2	9·371	1:36	0·002
45	0·08481	14·6	9·089	8·0	9·187	1:37	0·908
40	0·2182	14·4	9·257	7·8	8·988	1:38	0·82
38	0·3156	14·2	9·376	7·6	8·774	1:39	0·74
36	0·4532	14·0	9·491	7·4	8·546	1:40	0·67
34	0·6454	13·8	9·602	7·2	8·303	1:42½	0·52
32	0·9109	13·6	9·708	7·0	8·046	1:45	0·41
30	1·272	13·4	9·809	6·8	7·775	1:47½	0·32
29	1·497	13·2	9·860	6·6	7·489	1:50	0·25
28	1·756	13·0	9·937	6·4	7·175	1:55	0·154
27	2·205	12·8	10·08	6·2	6·881	1:60	0·094
26	2·354	12·6	10·16	6·0	6·559	1:65	0·057
25	2·775	12·4	10·23	5·8	6·225	1:70	0·035
25½	2·984	12·2	10·29	5·6	5·882	1:75	0·021
24	3·206	12·0	10·35	5·4	5·530	1:80	0·013
24·5	3·440	11·8	10·40	5·2	5·171	1:85	0·008
23	3·686	11·6	10·43	5·0	4·805	1:90	0·005
23·5	3·945	11·4	10·46			1:95	0·003
22	4·217	11·2	10·48	1:21	4·364	1:100	0·002
22·5	4·501	11·0	10·49	1:22	3·966		
21	4·797	10·8	10·49	1:23	3·59		
21·5	5·105	10·6	10·48	1:24	3·27		
20	5·424	10·4	10·46	1:25	2·96		
20·5	5·753	10·2	10·43	1:26	2·68		
19	6·090	10·0	10·38	1:27	2·44		
19·5	6·436	9·8	10·32	1:28	2·27		
18	6·787	9·6	10·25	1:29	2·00		
18·5	7·076	9·4	10·17	1:30	1·76		
17	7·499	9·2	10·07	1:31	1·64		
17·5	7·854	9·0	19·960	1:32	1·49		
16	8·205	8·8	9·834	1:83	1·34		
16·5	8·549	8·6	9·686	1:34	1·22		
15	8·885	8·4	954·0	1:35	1·11		

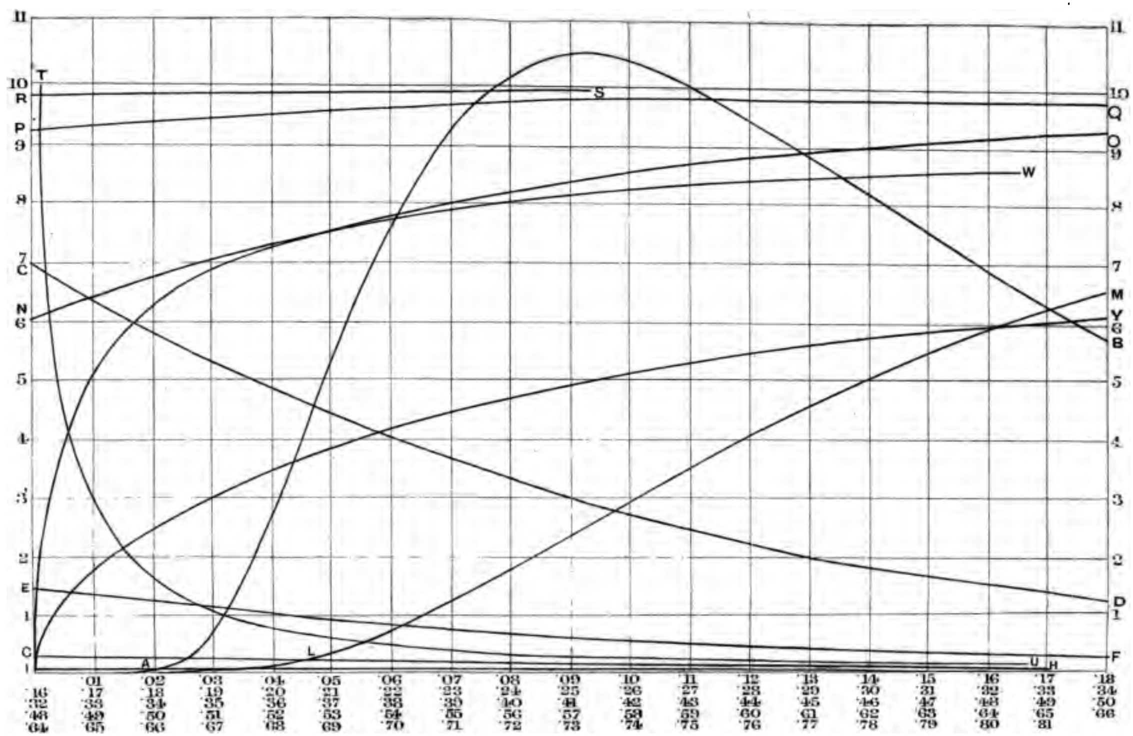

Fig. 3, Curve I

Thos Kell, Lithographer.

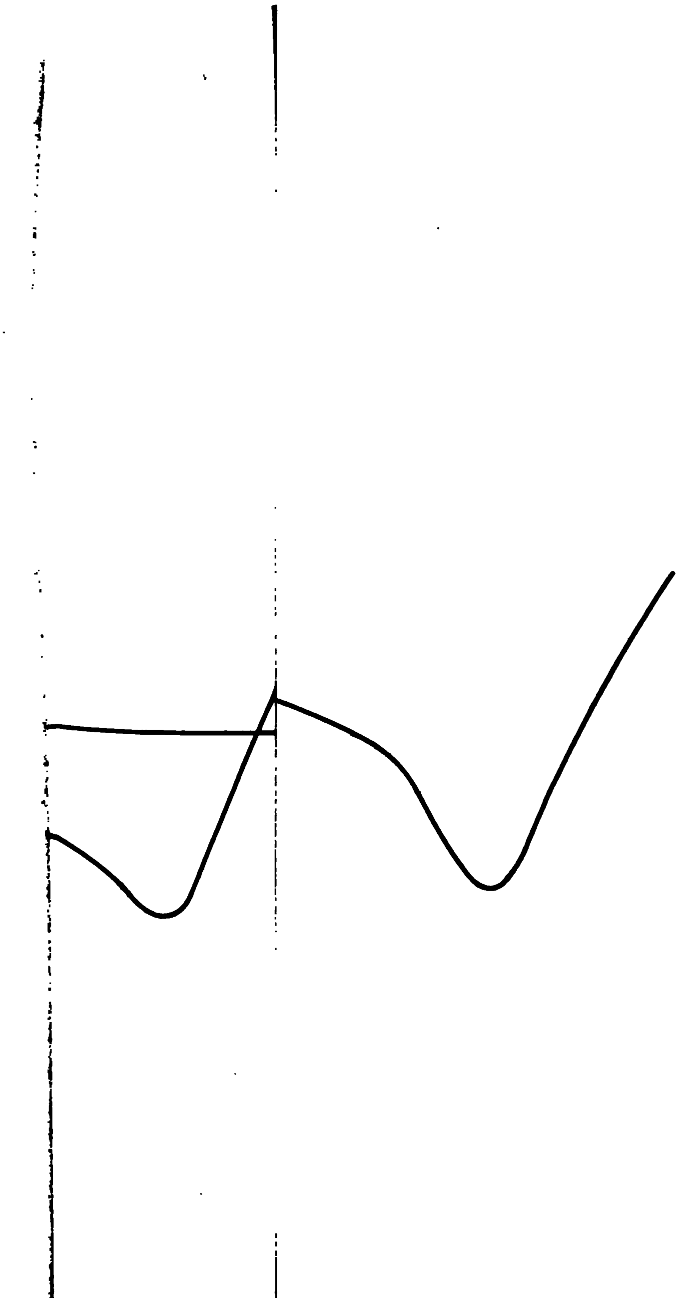

Fig. 4, Curve I

Thos. Kell, Lithographer.
40, King St. Covent Garden.

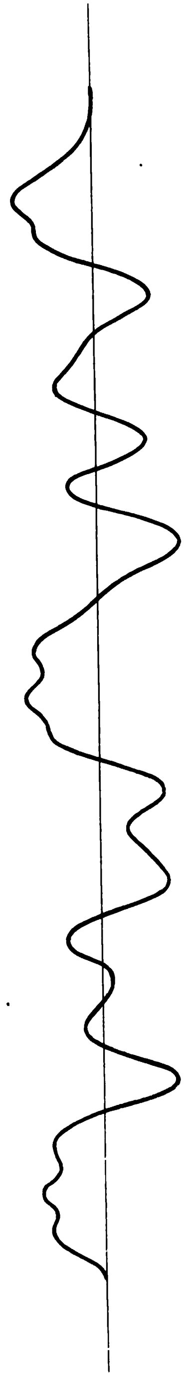

Fig 5 , Curve III

II. (fig. 4) and III. (fig. 5) have been drawn. Curve II. gives the form of the recorder signals for the signs "understand," A B C D at the rate of fifteen words per minute through 2,500 knots of cable of resistance 3 ohms and capacity 0·4 microfarads per knot. The curve of current for the case of direct signalling without condensers is also drawn. Curve III. gives the same for a speed of ten words per minute through the same cable. The first speed is perhaps as high as has ever been reached in practice. The

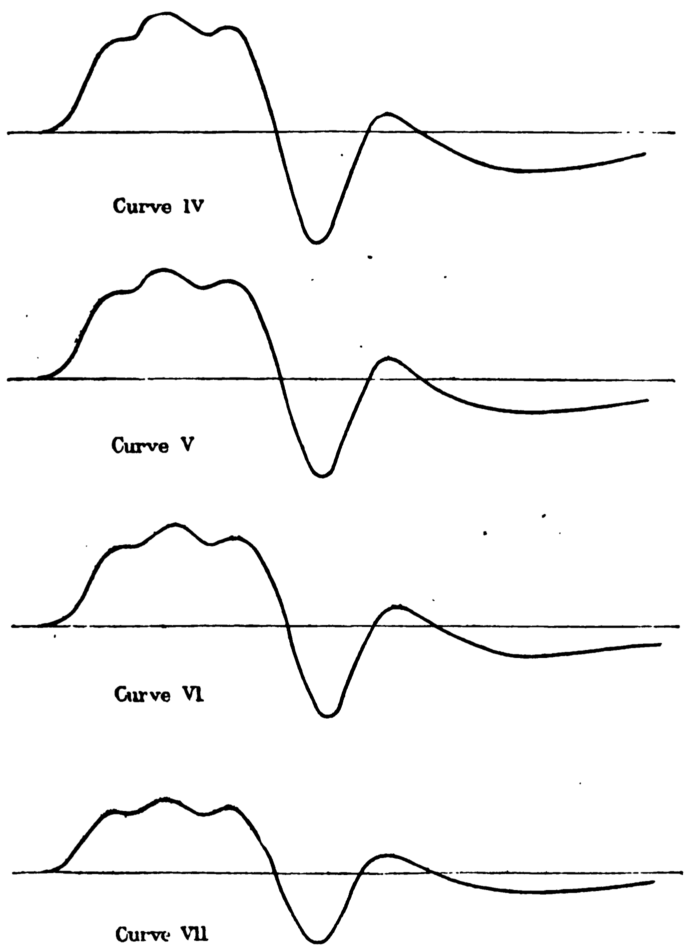

Fig. 6.

second speed will be found to be nearly the speed employed generally on long cables; it corresponds approximately to—

9 words	per minute	on the	French Atlantic cable,	
15	,,	,,	,,	1865 cable,
10	,,	,,	,,	Indian cables,
9	,,	,,	,,	China cable.

The theoretical curves are very similar to some recorder slips that I have seen from the Suez, Aden, and other cables.

Curves IV. V. VI. VII. (fig. 6) show the alteration in the shape of the signal caused by a shorter or longer battery contact, the speed being the same as in curve III. In IV. the battery contact is 0·0193 al^2, the earth 0·0155 al^2; in V. battery is 0·0174, and earth 0·0174; in VI. the battery contact is 0·0155 al^2, and the earth 0·0193 al^2; in VII. the battery contact is 0·0166 al^2, the earth 0·0232 al^2.

It must be remembered that these curves are the limiting curves as the sending condenser is diminished. The form of the signals on a recorder paper will differ somewhat, both on account of the larger condenser used and the viscosity of the instrument itself.

Returning to the equation giving the approximate law of the entering current. It gives the current entering as infinite at the first instant, because the resistance of the battery has been neglected, but the total quantity of electricity that has entered is given as finite as it should be. To find the total quantity of electricity that has entered the cable in time t we have only to integrate the equation, and we find the quantity (Q) given by the equation

$$Q = V \, . \, \lambda \, . \, c \left\{ \sqrt{\frac{t}{\pi a \lambda^2}} \left(1 - \epsilon^{-\frac{a\lambda^2}{t}}\right) + \left(1 - \frac{2}{\sqrt{\pi}} \int_0^z \epsilon^{-z^2} dz \right) \right\}$$

where z is written for $\frac{1}{\lambda} \sqrt{\frac{t}{a}}$.

The value of the unintegrated expression is given in the tables accompanying the article on the Theory of Probabilities in the Encyclopædia Metropolitana, and is too well known to require consideration here.

The following table gives (P) the proportion of the whole charge

entering the cable that has entered at the time τ from the commencement of the signal. τ is equal to $\frac{t}{a\lambda^2}$.

Value of τ	Value of P	Value of τ	Value of P	Value of τ	Value of P	Value of τ	Value of P	Value of τ	Value of P
0·000001	·000564	0·143	0·06743	1·818	0·2409	25·00	·6649	1429	·9652
0·000002	·000798	0·1667	0·07285	2·000	0·2534	33·33	·7053	1667	·9675
0·00001	·001784	0·2000	0·07979	2·500	0·2816	50·00	·7556	2000	·9703
0·00002	·002525	0·2500	0·08921	2·857	0·3006	100·	·8244	2500	·9633
0·0001	·005642	0·3333	0·1030	3·333	0·3238	111·1	·8332	3333	·9763
0·0004	·01128	0·5000	0·1262	4·000	0·3520	125	·8424	5000	·9867
0·001	·01784	1·000	0·1785	5·000	0·3904	142·9	·8525	10000	·9863
0·002	·05642	1·0526	0·1830	6·666	0·4411	166·7	·8625		
0·025	·02821	1·1111	0·1881	7·69	0·4661	200	·8749		
0·04	·03568	1·176	0·1935	9·091	0·4952	250	·8871		
0·05	·03989	1·250	0·1995	10·00	0·5139	333	·9028		
0·0667	·04606	1·333	0·2060	11·111	0·5332	500	·9205		
0·1	·05642	1·429	0·2134	12·5	0·5532	1000	·9437		
0·1111	·05947	1·538	0·2212	14·29	0·5758	1111	·9608		
0·125	·06308	1·667	0·2302	16·67	0·6015	1250	·9629		
		1·818		20·00	0·6312				

The curve corresponding to these numbers is shown in figure 1, where I γ is a portion of the curve, the scale of time being such that the distance between each of the thicker vertical divisions represents the interval τ or total resistance $\times$ total capacity of a length of cable, such that its capacity would equal the capacity of the sending condenser, and I W is the same curve, the scale for time being one-tenth of that just given.

The form of the curve is peculiar, it rises very rapidly for small values of τ and very slowly for larger values.

This curve is interesting in the theory of duplex telegraphy.

T U, fig. 1, is a curve calculated from equation (9); it shows the rate at which the *current* from the condenser enters the cable. The scale of time is one quarter that given above for curve I W, so that each of the thicker vertical divisions are supposed separated by an interval $\frac{2}{5}\,\tau$.

The ordinates are proportional to $4\sqrt{\pi} \times \frac{V}{\kappa\lambda}$ or $4\sqrt{\pi} \times$ the

current that the battery would maintain through a resistance equal to the resistance of a portion of the cable of which the capacity would equal the capacity of the sending condenser.

The following table gives the value of the current entering the cable at various intervals of time.

TABLE OF VALUE OF CURRENT ENTERING CABLE FROM SMALL SIGNALLING CONDENSER.

Value of τ	Value of γ	Value of τ	Value of γ
0·01	2·821	0·90	0·1995
0·02	1·995	1·00	0·1783
·03	1·629	1·1	0·1606
·04	1·410	1·2	0·1456
·05	1·262	1·3	0·1328
·06	1·152	1·4	0·1217
·07	1·066	1·5	0·1121
·08	0·9973	1·6	0·1036
·09	0·9403	1·7	0·0962
0·10	0·8921	1·8	0·0896
0·12	0·8141	1·9	0·0837
0·14	0·7533	2·0	0·0785
0·16	0·7035	2·2	0·06921
0·18	0·6623	2·4	0·06205
0·20	0·6265	2·6	0·05578
0·22	0·5964	2·8	0·05063
0·24	0·5669	3·0	0·04617
0·26	0·5414	3·2	0·04232
0·28	0·5181	3·4	0·03900
0·30	0·4967	3·6	0·03606
0·35	0·4494	3·8	0·03348
0·40	0·4095	4·0	0·03120
0·45	0·3749	5·0	0·02287
0·50	0·3449	6·0	0·01768
0·55	0·3186	7·0	0·01419
0·60	0·2954	8·0	0·01171
0·65	0·2748	9·0	0·00989
0·70	0·2564	10·0	0·00849
0·75	0·2396		
0·80	0·2250		
0·85	0·2111		

The accompanying table gives the value of γ for various values of τ or $a\lambda^2$. The unit of current is the current that the battery could maintain through a length of cable the capacity of which would equal the capacity of the sending condenser, which is supposed a very small fraction of the total capacity of the cable.

The corresponding current curve, when no condensers are in circuit but the cable is to earth at both ends, I have not drawn. Its equation is well-known. Adopting the same notation as before, we have

$$\gamma = \frac{V}{lk}\left(1 + 2\sum_{i=\infty}^{i=1} \epsilon^{-\frac{i^2\pi^2 t}{kcl^2}} \cos\frac{i\pi x}{l}\right) \quad . \quad . \quad . \quad 12.$$

Or, writing $x = O$, for the *sending* end of the cable,

$$\gamma = \frac{V}{lk}\left(1 + 2\sum_{i=\infty}^{i=1} \epsilon^{-\frac{i^2\pi^2 t}{kcl^2}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 13.$$

for the current entering the cable.

In Sir William Thomson's notation, where

$$\epsilon^{-\frac{\pi^2}{kcl^2} t} \text{ is written } 10^{-\frac{t}{10a}},$$

so that

$$a = \frac{\kappa c l^2}{\pi^2} \cdot \log_\epsilon 10^{\frac{1}{10}},$$

The equation takes the form—

$$\gamma = \frac{V}{lk}\left(1 + 2\left(10^{-\frac{1}{10} \cdot \frac{t}{a}} + 10^{-\frac{4}{10}\frac{t}{a}} + 10^{-\frac{9}{10}\frac{t}{a}} + \text{\&c.}\right)\right) \quad 14.$$

And the series is very readily calculated for integral values of $\frac{t}{a}$.

I am not aware that it has been remarked that each term of this series is positive, and therefore that the entering current can be represented as closely as we please by condensers and resistances. Let an arrangement be made as indicated on the following page.

The current entering a condenser of capacity s through a resistance r is at the time $t = \frac{V}{r} \epsilon^{-\frac{t}{sr}}$.

Compare this with the ith term in the value of γ equation (13), then $\frac{V}{r} = \frac{2V}{lk}$, $\frac{1}{sr} = \frac{i^2\pi^2}{kcl^2}$, or $r = \frac{1}{2} lk$, $s = \frac{2cl}{i^2\pi^2}$.

If therefore an arrangement similar to that drawn above is adopted, and the resistance in the branch A B is very small, and if

the resistance in the circuit A B D is the resistance of a cable, each of the resistances r_1, r_2, r_3 are half the resistance of the cable, and

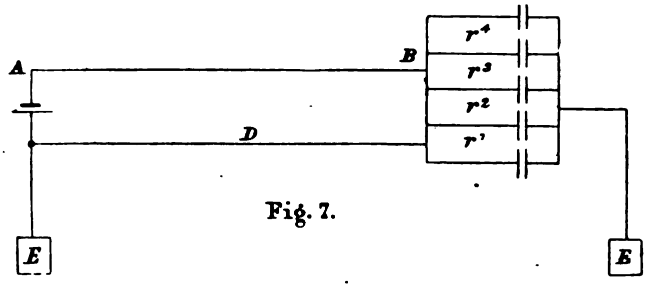

Fig. 7.

the capacities of the condensers are respectively the capacity of the cable divided by $\frac{\pi^2}{2}$, $4\frac{\pi^2}{2}$, $9\frac{\pi^2}{2}$ &c., then the current in A B is at each instant equal to the current entering a cable.

This suggests the best arrangement for duplexing a short cable or land-line *worked without condensers.*

Two, three, or more condensers in multiple arc, the resistances leading to them the same for all, and their capacities *diminishing* in the proportion of the reciprocals of the squares of the odd numbers. The total capacity in an infinite number of condensers would be

$$\frac{2\,c\,l}{\pi^2}\left(1 + \frac{1}{4} + \text{\&c.}\right) = \frac{2\,c\,l}{\pi^2}\frac{\pi^2}{6} = \frac{c\,l}{3},$$

or *one-third* the capacity of the cable.

In practice the resistance of A B, the receiving instrument, will be considerable, and this will alter the law somewhat, but the direction to commence in getting a balance is that stated, a few condensers in multiple arc, not in series.

If condensers are employed at both ends, we have seen that the form of the expression is totally different, and it would be necessary to calculate the proper values of C_1, C_2, &c., so that the total quantity that had entered the cable at various intervals (the intervals must be a small proportion of the time of vibration of the receiving instrument needle) should equal that given by curve I W at the same instant.

The various causes of disturbance above-mentioned, and the fact that the curve is only approximate when the condenser is small compared to the total capacity of the cable, would alter the values of C and r, so that in practice it would be necessary to find by trial the best arrangement starting with the theoretical arrangement as the basis. The artificial cable of Muirhead and Taylor is better in practice than a series of condensers, as it is theoretically perfect whatever connections are used for signalling.

We may note that equation (9) does not involve the length of the cable at all, because it is an approximation in which the fraction $\frac{\lambda}{l}$ has been neglected. Had the next term of the series been retained it would have been (within the bracket),

$$4\lambda^2 \sum_{n=\infty}^{n=1} \psi a(2nl),$$

or

$$= \frac{4V\lambda^2}{2\kappa\sqrt{\pi}} \sum_{n=\infty}^{n=1} \sqrt{a}\left(\frac{n^2 a^2 l^2}{t^2\sqrt{t}} - \frac{a}{2t\sqrt{t}}\right)\epsilon^{-\frac{n^2 a l^2}{t}}.$$

When t is only a few multiples of $a\lambda^2$, it will be a small fraction of $a\,l^2$, and therefore the value of this term will be small. It follows, that in working a *long* cable on the duplex method a very close imitation of the entering current can be obtained with an artificial cable of considerably shorter length than the actual cable to be imitated.

[Verbatim copy of Professor Œrsted's original communication on his discovery of Electro-Magnetism.]

EXPERIMENTA CIRCA EFFECTUM CONFLICTUS ELECTRICI IN ACUM MAGNETICAM.

Prima experimenta circa rem, quam illustrare aggredior, in scholis de Electricitate, Galvanismo et Magnetismo proxime-superiori hieme a me habitis instituta sunt. His experimentis monstrari videbatur, acum magneticam ope apparatus galvanici e situ

moveri; idque circulo galvanico cluso, non aperto, ut frustra tentaverunt aliquot abhinc annis physici quidam celeberrimi. Cum autem hæc experimenta apparatu minus efficaci instituta essent, ideoque phænomena edita pro rei gravitate non satis luculenta viderentur, socium adscivi amicum Esmarch, regia consiliis justiciæ, ut experimenta cum magno apparatu galvanico, a nobis conjunctim instructo repeterentur et augerentur. Etiam vir egregius Wleugel, eques auratus ord. Dan. et apud nos præfectus rei gubernatoriæ, experimentis interfuit, nobis socius et testis. Præterea testes fuerunt horum experimentorum vir excellentissimus et a rege summis honoribus decoratus *Hauch*, cujus in rebus naturalibus scientia jam diu inclaruit, vir acutissimus Reinhardt, Historiæ naturalis Professor, vir in experimentis instituendis sagacissimus Jacobsen, Medicinæ Professor, et Chemicus experientissimus Zeise, Philosophiæ Doctor. Sæpius equidem solus experimenta circa materiam propositam institui, quæ autem ita mihi contigit detegere phænomena, in conventu horum virorum doctrissimorum repetivi.

In experimentis recensendis omnia præteribo, quæ ad rationem rei inveniendam quidem conduxerunt, hac autem inventa rem amplius illustrare nequeunt; in eis igitur, quæ rei rationem perspicue demonstrant, acquiescamus.

Apparatus galvanicus, quo usis summus,* constat viginti receptaculis cupreis rectangularibus, quorum et longitudo et altitudo duodecim æqualiter est pollicum, latitudo autem duos pollices et dimidium vix excedit. Qvodvis receptaculum duabus laminis cupreis instructum est ita inclinatis, ut baculum cupreum, qui laminam zinceam in aqua receptaculi proximi sustentat, portare possint. Aqua receptaculorum $\frac{1}{60}$ sui ponderis acidi sulphurici et pariter $\frac{1}{60}$ acidi nitrici continet. Pars cujusque laminæ Zinceæ in aqua submersa Qvadratum est, cujus latus circiter longitudinem 10 pollicum habet. Etiam apparatus minores adhiberi possunt, si modo filum metallicum candefacere valeant.

Conjungantur termini oppositi apparatus galvanici per filum metallicum, quod brevitatis causa in posterum conductorem conjungentem vel etiam filum conjungens appellabimus. Effectui autem

* Sic. Lege "usi sumus."

qui in hoc conductore et in spatio circumjacente locum habet, conflictus electrici nomen tribuemus.

Ponatur pars rectilinea hujus fili in situ horizontali super acum magneticam rite suspensam, eique parallela. Si opus fuerit, filum conjungens ita flecti potest, ut pars eius idonea situm ad experimentum necessarium obtineat. His ita comparatis, acus magnetica movebitur, et quidem sub ea fili conjungentis parte, quæ electricitatem proxime a termino negativo apparatus galvanici accipit, occidentem versus declinabit.

Si distantia fili conjungentis ab acu magnetica $\frac{3}{4}$ pollices non excedit, declinatio acus angulum circiter 45° efficit. Si distantia augetur, anguli decrescunt ut crescunt distantiæ. Cæterum declinatio pro efficacia apparatus varia est.

Filum conjungens locum mutare potest vel orientem vel occidentem versus dummodo situm acui parallelum teneat, sine alia effectus mutatione, quam respectu magnitudinis; itaque effectus attractioni minime tribui potest, nam idem acus magneticæ polus, qui ad filum conjungens accedit, dum ei ad latus orientale positum est, ab eadem recedere deberet, quando locum ad latus occidentale occupat, si hæ declinationes ab attractionibus vel repulsionibus penderent. Conductor conjungens e pluribus filis aut tæniis metallicis connexis constare potest. Natura metalli effectus non mutat, nisi forte quoad qvantitatem. Fila ex platino, auro, argento, orichalco, ferro, tænias e plumbo et stanno, massam hydrargyri æquali cum successu adhibuimus. Conductor aqua interrupta non omni effectu caret, nisi interruptio spatium plurium pollicum longitudinis complectatur.

Effectus fili conjungentis in acum magneticam per vitrum, per metalla, per lignum, per aquam, per resinam, per vasa figlina, per lapides transeunt; nam interjecta tabula vitrea metallica vel lignea minime tolluntur, nec tabulis ex vitro, metallo et ligno simul interjectis evanescunt, imo vix decrescere videntur. Idem est eventus, si interjicitur discus electrophori, tabula ex porphyrita, vas figlinum, si vel aqua repletum sit. Experimenta nostra etiam docuerunt, effectus jam memoratos non mutari, si acus magnetica pyxide ex orichalco aqua repleta includitur. Effectuum transitum per omnes has materias in electricitate et galvanismo antea nunquam obser-

vatum fuisse, monere haud opus est. Effectus igitur, qui locum habent in conflictu electrico, ab effectibus unius vel alterius vis electricæ quam maxime sunt diversi.

Si filum conjungens in plano horizontali sub acu magnetica ponitur, omnes effectus idem sunt ac in plano super acum tantummodo in directione inversa. Acus enim magneticæ polus, sub quo ea est fili conjungentis pars, quæ electricitatem proxime a termino negativo apparatus galvanici accipit, orientem versus declinabit.

Ut facilius hæc memoria retineantur, hac formula utamur: Polus super quem intrat electricitas negativa ad occidentem, infra quam ad orientem vertitur.

Si filum conjungens in plano horizontali ita vertitur, ut cum meridiano magnetico angulum sensim sensimque crescentem formet, declinatio acus magneticæ augetur, si motus fili tendit versus locum acus deturbatæ; sed minuitur, si filum ab hoc loco discedit.

Filum conjungens in plano horizontali, in quo movetur acus magnetica, ope sacomatis æquilibrata, situm, et acui parallelum, eandem nec orientem nec occidentem versus deturbat, sed tantummodo in plano inclinationis nutare facit, ita ut polus, penes quem ingreditur in filum vis negative electrica deprimatur, quando ad latus occidentale, et elevetur, quando ad orientale situm est.

Si filum conjungens perpendiculare ad planum meridiani magnetici, vel supra vel infra acum ponitur, hæc in quiete permanet; excepto si filum sit polo admodum propinquum: tum enim elevatur polus, quando introitus fit a parte occidentali fili, et deprimitur quando ab orientali fit.

Quando filum conjungens prependiculare ponitur e regione polo acus magneticæ, et extremitas superior fili electricitatem a termino negativo apparatus galvanici accipit, polus orientem versus movetur; posito autem filo e regione puncto inter polum et medium acus sito, occidentem versus agitur. Quando extremitas fili superior electricitatem a termino positivo accipit phænomena inversa occurrent.

Si filum conjungens ita flectitur, ut ad ambas flexuræ partes sibi fiat parallelum, aut duo formet crura parallela, polos magneticos pro diversis rei conditionibus repellit aut attrahit. Ponatur filum

e regione polo alteriutri acus, ita ut planum crurum parallelorum sit ad meridianum magneticum perpendiculare et conjugatur crus orientale cum termino negativo, occidentale cum positivo apparatus galvanici; quibus ita instructis, polus proximus repelletur, vel ad orientem vel ad occidentem pro situ plani crurum. Conjuncto crure orientali cum termino positivo et occidentali cum termino negativo, polus proximus attrahitur. Quando planum crurum ponitur perpendiculare ad locum inter polum et medium acus, iidem, tantummodo inversi, occurrunt effectus.

Acus ex orichalco, ad instar acus magneticæ suspensa, effectu fili conjungentis non movetur. Etiam acus ex vitro, vel ex sic dicto gummi lacca, simili experimento subjectæ in quiete manent.

Ex his omnibus momenta quædam ad rationem horum phænomenorum reddendam afferre liceat.

Conflictus electricus non nisi in particulas magneticas materiæ agere valet. Videntur omnia corpora non-magnetica per conflictum electricum penetrabilia esse; magnetica vero, aut potius particulæ eorum magneticæ transitui hujus conflictus resistere, quo fit, ut impetu virium certantium moveri possint.

Conflictum electricum in conductore non includi, sed, ut jam diximus, simul in spatio circumjacente idque satis late dispergi, ex observationibus jam propositis satis patet.

Similiter ex observatis colligere licet, hunc conflictum gyros peragere, nam hæc esse videtur conditio, sine qua fieri nequeat, ut eadem pars fili conjungentis, quæ infra polum magneticum posita eum orientem versus ferat, supra posita eundem occidentem versus agat; hæc enim gyri est natura, ut motus in partibus oppositis oppositam habeant directionem. Præterea motus per gyros cum motu progressivo, juxta longitudinem conductoris, conjunctus, cochleam vel lineam spiralem formare debere videtur, quod tamen, nisi fallor, ad phænomena hucusque observata explicanda nihil confert.

Omnes in polum septentrionalem effectus, hic expositi, facile intelliguntur, ponendo, vim vel materiam negative electricam lineam spiralem dextrorsum flexam percurrere, et polum septentrionalem propellere, in meridionalem autem minime agere. Effectus in polum meridionalem similiter explicantur, si vi vel materiæ positive electricæ motum contrarium et facultatem in polum meridionalem non autem in septentrionalem agendi tribuimus.

Hujus legis cum natura congruentia melius repetitione experimentorum quam longa explicatione perspicietur. Dijudicatio autem experimentorum multo fiet facilior, si cursus virium electricarum in filo conjungente signis pictis vel incisis indicatus fuerit.

Dictis hoc tantum adjiciam: Demonstrasse me in libro septem abhinc annis edito, calorem et lucem esse conflictum electricum. Ex observationibus nuper adlatis jam concludere licet, motus per gyros etiam in his effectibus occurrere; quod ad phænomena, quæ polaritatem lucis appellant, illustranda perquam facere puto.

Dabam Hafniæ d. 21de Julii 1820.

Johannis Christianus Örsted.

Eques auratus Ordinis Dannebrogici in Universitate Hafniensi Prof. Physices Ord., Secretarius Societatis Regiæ Scientiarum Hafniensis.

TYPIS *SCHULTZIANIS.*

(*Translation*).*

EXPERIMENTS ON THE EFFECT OF ELECTRIC ACTION ON THE MAGNETIC NEEDLE.

The first experiments on the subject which I undertake to illustrate were set on foot in the classes for electricity, galvanism, and magnetism, which were held by me in the winter just past. By these experiments it seemed to be shown that the magnetic needle was moved from its position by the help of a galvanic apparatus, and that, when the galvanic circuit was closed, but not when open, as certain very celebrated physicists in vain attempted several years ago. As, however, these experiments were conducted with somewhat defective apparatus, and, on that account, the phenomena which were produced did not seem clear enough for the importance of the subject, I got my friend Esmarch, the King's Minister of Justice, to join me, that the experiments might be repeated and extended with the great galvanic apparatus which we fitted up together. A distinguished man, Wleugel, Knight of the Danish Order, and President of our Pilot Board, was also present at our experiments as a partner and a witness. Besides these there were witnesses at these experiments that most excellent man, decorated by the King with

* The Society is indebted for this Translation to the Rev. J. E. Kempe, Rector of St. James's, Piccadilly, &c., &c., to whom it accords its thanks.

the highest of honours,—Hauch, whose acquaintance with natural science has long been celebrated,—that most acute man Reinhardt, Professor of Natural History; Jacobsen, Professor of Medicine, a man of the utmost sagacity in conducting experiments; and the most experienced chemist, Zeise, Doctor of Philosophy. I have indeed somewhat frequently carried out by myself experiments relating to the matter proposed, but the phenomena which it thus befel me to disclose I repeated in the presence of these most learned men.

In reviewing my experiments I will pass over everything which, though they conduced to the discovery of the reason of the thing, yet, when this is discovered, cannot any further illustrate it. Those things, therefore, which clearly demonstrate the reason of the thing, let us take for granted.

The galvanic apparatus which we made use of consists of 20 rectangular copper receptacles, the length and height of which are alike 4 inches, the breadth, however, scarcely exceeding $2\frac{1}{2}$ inches. Every receptacle is furnished with two copper plates, so inclined that they can carry a copper bar which supports a zinc plate in the water of the next receptacle. The water of the receptacles contains $\frac{1}{60}$ of its weight of sulphuric acid and likewise $\frac{1}{60}$ of its weight of nitric acid. The part of each plate which is immersed in the solution is square, the side being about 10 inches long. Even smaller apparatus may be used, provided they are able to make a metallic wire red hot.

Let the opposite poles of the galvanic apparatus be joined by a metallic wire, which, for brevity, we will call hereafter the joining conductor or else the joining wire. To the effect, however, which takes place in this conductor and surrounding space, we will give the name of electric conflict.

Let the rectilinear part of this wire be placed in a horizontal position over the magnetic needle duly suspended, and parallel to it. If necessary, the joining wire can be so bent that the suitable part of it may obtain the position necessary for the experiment. These things being thus arranged, the magnetic needle will be moved, and indeed, under that part of the joining wire which receives electricity most immediately from the negative end of the galvanic apparatus, will decline towards the west.

If the distance of the joining wire from the magnetic needle does not exceed ¾ of an inch, the declination of the needle makes an angle of about 45°. If the distance is increased the angles decrease as the distances increase. The declination, however, varies according to the efficiency of the apparatus.

The joining wire can change its place either eastward or westward, provided it keeps a position parallel to the needle, without any other change of effect than as respects magnitude; and thus the effect can by no means be attributed to attraction, for the same pole of the magnetic needle which approaches the joining wire while it is placed at the east side of it ought to recede from the same when it occupies a position at the west side of it if these declinations depended upon attractions or repulsions.

The joining conductor may consist of several metallic wires or bands connected together. The kind of metal does not alter the effects, except, perhaps, as regards quantity. We have employed with equal success wires of platinum, gold, silver, copper, iron, bands of lead and tin, a mass of mercury. A conductor is not wholly without effect when water interrupts, unless the interruption embraces a space of several inches in length.

The effects of the joining wire on the magnetic needle pass through glass, metal, wood, water, resin, earthenware, stones; for if a plate of glass, metal, or wood be interposed, they are by no means destroyed, nor do they disappear if plates of glass, metal, and wood be simultaneously interposed; indeed, they seem to be scarcely lessened. The result is the same if there is interposed a disc of amber, a plate of porphyry, an earthenware vessel, even if filled with water. Our experiments have also shown that the effects already mentioned are not changed if the magnetic needle is shut up in a copper box filled with water. It is unnecessary to state that the passing of the effects through all these materials in electricity and galvanism has never before been observed. The effects, therefore, which take place in electric conflict are as different as possible from the effects of one electric force or another.

If the joining wire is placed in a horizontal plane under the magnetic needle, all the effects are the same as in the plane over the needle, only in an inverse direction, for the pole of the magnetic needle under which is that part of the joining wire

which receives electricity most immediately from the negative end of the galvanic apparatus will decline towards the east.

That these things may be more easily remembered let us use this formula: the pole *over* which negative electricity enters is turned towards the west, that *under* which it enters towards the east.

If the joining wire is so turned in a horizontal plane as to form with the magnetic meridian a gradually increasing angle, the declination of the magnetic needle is increased if the motion of the wire tends towards the place of the disturbed needle, but is lessened if the wire goes away from this place.

The joining wire placed in the horizontal plane in which the magnetic needle moves balanced by means of a counterpoise, and parallel to the needle, disturbs the same neither eastward nor westward but only makes it quiver in the plane of inclination, so that the pole near which the negative electric force enters the wire is depressed when it is situated at the west side and elevated when at the east.

If the joining wire is placed perpendicular to the plane of the magnetic meridian, either above or below the needle, the latter remains at rest, unless the wire is very near to the pole, for then the pole is elevated when the entrance is made from the western part of the wire and depressed when it is made from the eastern.

When the joining wire is placed perpendicular to the pole of the magnetic needle, and the upper end of the wire receives electricity from the negative end of the galvanic apparatus, the pole is moved towards the east; but when the wire is placed opposite to a point situated between the pole and the middle of the needle it is driven towards the west. When the upper end of the wire receives electricity from the positive end reverse phenomena will occur.

If the joining wire is so bent that it is made parallel to itself at both parts of the bend, or forms two parallel legs, it repels or attracts the magnetic poles according to the different conditions of the case. Let the wire be placed opposite to either pole of the needle so that the plane of the parallel legs is perpendicular to the magnetic meridian, and let the eastern leg be joined with the negative end of the galvanic apparatus, the western with the positive, and when this is so arranged the nearest pole will be repelled

either eastward or westward according to the position of the plane of the legs. When the eastern leg is joined with the positive end, and the western with the negative, the nearest pole is attracted. When the plane of the legs is placed perpendicular to a spot between the pole and the middle of the needle the same effects occur, only inverted.

A needle of copper, suspended like a magnetic needle, is not moved by the effect of a joining wire. Also needles of glass, or of so-called gum-lac, subjected to the like experiments, remain at rest.

From all this it may be allowable to adduce some considerations in explanation of these phenomena.

Electric conflict can only act upon magnetic particles of matter. All non-magnetic bodies seem to be penetrable through electric conflict; but magnetic bodies, or rather their magnetic particles, seem to resist the passage of this conflict, whence it is that they can be moved by the impulse of contending forces.

That electric conflict is not inclosed in the conductor, but as we have already said is at the same time dispersed in the surrounding space, and that somewhat widely is clear enough from the observations already set forth.

In like manner it is allowable to gather from what has been observed that this conflict performs gyrations, for this seems to be a condition without which it is impossible that the same part of the joining wire, which, when placed beneath the magnetic pole, carries it eastward, drives it westward when placed above; for this is the nature of a gyration, that motions in opposite parts have an opposite direction. Moreover, motion by circuits combined with progressive motion, according to the length of the conductor, seems bound to form a cochlea or spiral line, which, however, if I am not mistaken, contributes nothing to the explanation of phenomena hitherto observed.

All the effects on the northern pole, here set forth, are easily understood by stating that negatively electric force or matter runs through a spiral line bending to the right, and propels the northern pole, but does not act at all upon the southern. The effects on the southern pole are similarly explained if we attribute to force or matter positively electrified a contrary motion and the power of acting

on the southern pole but not on the northern. The agreement of this law with nature will be better seen by the repetition of experiments than by a long explanation. To judge of the experiments, however, will be made much easier if the course of the electric force on the joining wire is indicated by marks, either painted or incised.

I will add this only to what has been said: that I have demonstrated in a book, published seven years ago, that heat and light are in electric conflict. From observations lately brought to bear we may now conclude that motion by gyrations also occurs in these effects; and I think that this does very much to illustrate the phenomena which they call the polarity of light.

JOHN CHRISTIAN ÖRSTED,
Knight of the Dannebrogic Order,
Professor of Physics in the Copenhagen University,
Secretary to the Copenhagen Royal Society of Sciences.

Dated, Copenhagen,
21 July, 1820.

The following is a fac-simile of Professor Œrsted's signature, taken from a communication signed by him, addressed to the Secretary of the London Electrical Society, bearing date the 9th of April, 1844, and presented to the Society by Mr. C. V. Walker, F.R.S.

HANS CHRISTIAN ŒRSTED.

On the 25th September, 1876, the statue erected to the memory of Hans Christian Œrsted at Copenhagen was unveiled in the presence of His Majesty the King of Denmark and the King of Greece, the Danish Crown Prince, and several members of the royal Danish family, and a large assemblage of high officials, men of science, and students, surrounded by thousands of people wish-

ing to pay a tribute to the memory of the celebrated philosopher. To the members of the Society of Telegraph Engineers the celebration in memory of this eminent man must prove peculiarly interesting, as he, by the discovery of electro-magnetism, laid the foundation of the electric telegraph. It is thought, therefore, that a short sketch of his life will not prove unwelcome.

H. C. Œrsted was born in the town of Rudkjöbing, on the island of Langeland, at the Great Belt, on the 11th of August, 1777. He and his younger brother, the since famous jurist and statesman, Anders Sandöe Œrsted, received in their childhood a rather indifferent education, but by their eagerness and thirst for knowledge, as well as from their remarkable natural gifts, they were so far successful that they, after a short time of instruction in Copenhagen, especially in the classical languages, could already, in 1794, submit themselves to the academical examination and be accepted at the university. The following year H. C. Œrsted passed the philosophical examination, and in 1797 the pharmaceutical in a brilliant manner. A strong love and sense for the study of natural science was early awakened in him, the first impulse to which was perhaps given when as a boy of thirteen years of age he was apprenticed to his father, who was an apothecary; but, besides natural science, he occupied himself with great interest with æsthetics and philosophy. In 1796 he won an academical prize for an æsthetic treatise, and in 1798 for a medical one. In 1799 he was created Doctor of Philosophy, after having defended his "Dissertatis de forma metaphysices elementaris naturæ externæ." Whilst abroad Œrsted published at Berlin his first great work, which afterwards appeared in French under the title of "Recherches sur l'identité des forces electriques et chemiques." In this work is to be found the foundation of the electro-chemical system, which the famous Swedish chemist Berzelius afterwards more fully developed. Œrsted was at that time, and perhaps not entirely without justice, looked upon by many more as a natural philosopher than as one devoted to the study of natural science; but later, it clearly appears from his works that he more and more occupied himself with trying experiments, which soon led him to the highest renown and secured for

his name a prominent place in the history of science. In 1818 he began to make investigations on the compression of water, and constructed for this purpose an apparatus which is now to be found in all physical collections, and is as remarkable for its elegance as its correctness. In the same year he improved the galvanic trough apparatus, making many interesting experiments by its means, the most noticeable of which, perhaps, was his employment of it for the explosion of mines by heating a wire placed in gunpowder; but it was not until two years later that he realised the idea which early occupied, and in fact filled his mind, viz., that of finding the true connection between electricity and magnetism, by the action of the galvanic current on the magnetic needle. In July, 1820, his labours in this direction were brought to a conclusion, and on the 21st of July, 1820, he published his discovery in his treatise, "Experimenta circa effectum conflictus electrici in acum magneticam," which was sent to all of the most renowned natural philosophers and scientific societies.

From this moment his great worth as a natural scientist was universally acknowledged, and honours poured thick upon him from all quarters. He became member and honorary member of a multitude of learned societies. In 1823 he was created Member of the Royal Society and Honorary Member of the Royal Institution of Great Britain, and Corresponding Member of l'Institut de France (1872 Associé étranger).

What immense influence Œrsted's discovery exercised, and how it was further developed, partly in a theoretical direction and partly in a practical direction, need not here be referred to; it will in this and other respects be sufficient to observe that the total of all his treatises and publications reached the fair number of 218, embracing the whole field of natural science.

The last work published by Œrsted, and no doubt the most important, as an expression of that view of the world which he entertained from his earliest time, and in which he by degrees became only more confirmed, was his work, "the Soul of Nature." For him Nature was a manifestation of the Deity's combined wisdom and creative power. The laws of Nature are reason's laws. The

true and the beautiful are but different views of what is rational, and the whole existence forms an all-comprehensive rational whole of Divine origin.

Particularly for his own country did Œrsted work in a beneficial manner, not only as a teacher at the University, where his warm and animated lectures enraptured his audience, and won many votaries for the study of Nature, but his interest was as many-sided as his learning, and in many instances have his lectures and writings given instruction and guidance to his fellow-citizens. He worked energetically for the purpose of making the science of Nature more accessible to the public at large, as well as that the influence of science on daily life should be developed in his own country in a practical direction. It was with this idea that he founded the Society for Advancement of Natural Science, and that he succeeded in 1829 in establishing the Polytechnic Institution at Copenhagen, for which he, as director and teacher, worked with untiring energy up to his death.

On the 7th of November, 1850, he had been teacher at the University for fifty years, and a large number of his friends, pupils, and hearers prepared a festival for him on that day. Honoured by his King, loved and respected by all who had experienced the good fortune of coming in contact with him, Œrsted looked upon that day as the happiest in his life. A few months later, on the 9th of March, 1851, after an apparently light illness, he passed quietly away, deeply mourned by all. Eminent as a scholar, equally great was he as a man, modest and lenient in his judgment of others, strict with regard to himself, benevolent, always ready to help others with advice and deed; himself truthful in the highest degree, he demanded truthfulness from others as their first duty.

Œrsted visited England several times, and he was on a friendly footing with most of its renowned scientific men. In 1823 he made the acquaintance of Davy, Wollaston, and others; in 1846 he took part in the meeting of Natural Philosophers at Southampton, and on that occasion he was particularly distinguished. Amongst others Sir John Herschel spoke at the concluding meeting in the most

flattering terms of Œrsted and his labours. He made and renewed at that time acquaintance with many Englishmen, such as Faraday, Murchison, Wheatstone, and others. Wheatstone particularly entertained a great esteem for, and often spoke with the highest regard of, his elder friend, the Danish philosopher.

It is impossible to conclude these lines, devoted to the memory of a man of classical character, distinguished by his love to his country, his science, and his family, without showing in a few words how his genius, a quarter of a century after his death, still exercises its influence on that nation to which he belonged. On the 25th of September, the same day as the monument for Œrsted was uncovered, and in memory of that day, the brewer, Mr. Jacobsen of Carlsberg, by a donation of one million Danish crowns, established the Carlsberg fund "for the advancement of science, and for the benefit of scientific men," thus, as the high-minded donor expresses himself, discharging only partially the debt in which he stands to Œrsted and science. And once more we meet the genius of Œrsted in that warm interest with which the Danish nation embraces the grand schemes carried out by the Great Northern Telegraph Company, under the presidency of Mr. Tietgen of Copenhagen.

C. L. MADSEN.

Copenhagen, October, 1876.

ON DUPLEX TELEGRAPHY,

BY J. J. FAHIE.

The following is an abstract of an interesting Paper forwarded to us by Mr. Fahie, which is too long for insertion and not sufficiently novel for reading before the Society.

Acting upon a system of testing devised by Mr. Mance, and described in the "Philosophical Magazine" for April, 1871, which briefly described is an arrangement such as that shown in fig. 1, a point P in the shunt A B can be found experimentally at which

earth may be applied without affecting the deflection of the galvanometer needle G; when P is thus found the resistances A B E and R will be proportional, that is

$$\frac{A}{B} = \frac{E}{R}.$$

The application of earth at the point P, while leaving the galvanometer undisturbed, must considerably modify the current set up in R by the electromotive E, and here we have the first step towards a duplex telegraph.

Apparatus was arranged as in fig. 2. A B, A_1 B_1, and R were resistance coils of one form or another, G G_1 were polarised relays, and were so connected up that when the current flowed through them the tongue of G was attracted to the contact stop, while that of G_1 was repelled. In the circuit of each relay there was a fairly sensitive galvanoscope not shown in the figure. K K_1 were ordinary Morse keys, E was a battery of 30 cells of small internal resistance, and L was the artificial line of about 3000 ohms. The local circuits differed in no way from those for ordinary single working, and, for the sake of clearness, are omitted from the figure.

Fair workable results were obtained when $A + B$ was equal to the resistance of the battery E added to that of the line and of the distant station's apparatus, $A_1 + B_1 = 2\ G_1$, and R equal to the line and the home station's apparatus. The resistance of the relay circuits was about 800 ohms.

The adjustment was effected in the following manner. The tongue of G was made to rest well against the insulated or agate stop, so that the current passing through the system should be unable to draw it over to the contact stop. The tongue of G, on the other hand, was so placed as to rest against the contact stop when no current was passing, and to be pushed against the insulated stop by the current from E. The wires from K K_1, being fitted with travelling pegs, were then moved along A B and A_1 B_1 until a point was found in each at which making and breaking contact with earth by means of the levers had no effect on the deflections of their corresponding galvanoscopes, that is to say, working K had

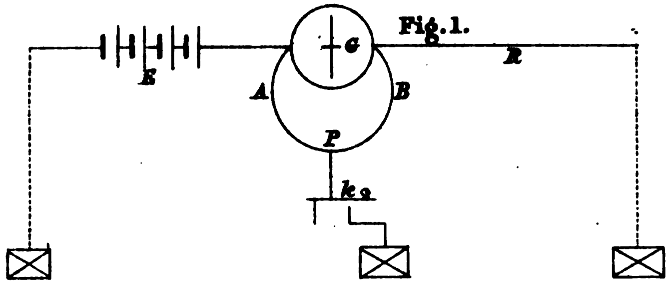

Fig. 1.

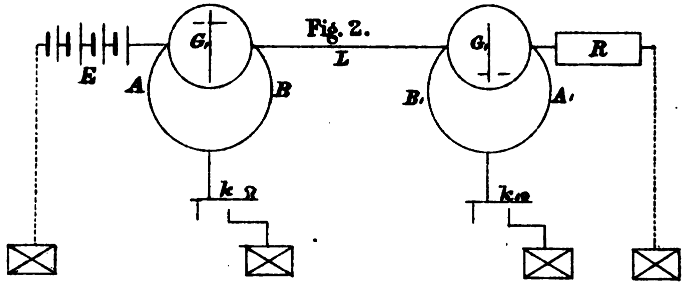

Fig. 2.

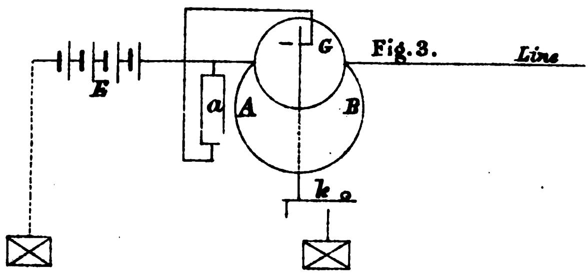

Fig. 3.

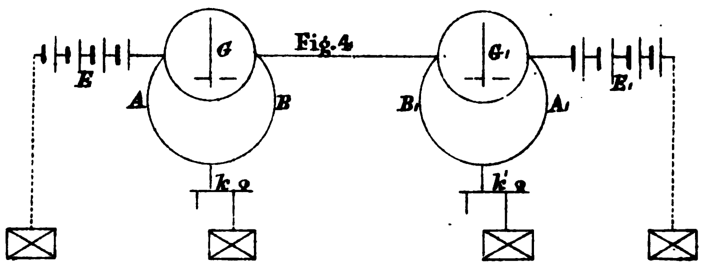

Fig. 4

no effect on G, and working K_1 none on G_1; when this was so

$$\frac{A}{B} = \frac{E}{L+Y+R} \text{ and } \frac{A_1}{B_1} = \frac{R}{L+X+E},$$

Y being the combined resistance of the distant relay and shunt, and X that of the home relay and shunt.

The *modus operandi* may be briefly explained thus:—When both keys were at rest the relays were open; G, because of its adjustment the current was too weak to attract the tongue, and G_1, because we so arranged that the same current should be able to keep the tongue against the insulated stop. Now, when K was depressed, the greater part of the current leaving the battery was diverted to earth *viâ* A and K, and G B and K, while that portion of it in the line was reduced to about one-half of its original strength, and was no longer able to keep the tongue of G_1 from falling over to the contact stop. Whenever, therefore, K was depressed, G_1 recorded a signal. When K_1 was held down the resistance of the circuit was halved, roughly speaking, and the current flowing out to line was in consequence doubled, and so was strong enough to close the relay G and make a signal at that station.

When the keys were worked singly the signals at either end were perfect, but when both were worked at the same time the signals at the battery end were disturbed and sometimes broken, while those at the distant station continued good. This was, of course, owing to the derangement of the proportionality on which the system is based, for every time K was held down R was cut out of circuit and the rates $\frac{A}{B} = \frac{E}{L+Y+R}$ were no longer correct. To remedy this an automatic contrivance was introduced in G, by which, when K_1 was depressed, the value of A was doubled. Fig. 3 illustrates this. While the relay G was open, the resistance marked A in figure 2 was really the joint resistance of two equal quantities, A and a. When G closed, in response to the working of K_1, it broke the circuit of a, and so doubled the value of this portion of the shunt. In this way, whether K_1 were up or down, the proportionality of all the parts was fairly maintained.

Thus we had when K_1 was at rest,

$$\frac{A}{B} = \frac{E}{L+Y+R},$$

and when K_1 was depressed,

$$\frac{2A}{B} = \frac{E}{\frac{L+Y+R}{2}} \text{ or } \frac{A}{B} = \frac{E}{L+Y+R} \text{ as before.}$$

This compensation was not required when K was depressed, because the decrease in the resistance of the home apparatus caused thereby was so small compared to the total resistance that the ratio $\frac{A_1}{B_1} = \frac{R}{L+X+E}$ was practically correct for this position of the lever.

Subsequent experiments showed that the resistance R could be conveniently replaced by a second battery E, fig. 4, and the automatic arrangement dispensed with. This battery was equal, approximately, in internal resistance and electromotive force to E, and was connected up in the opposite way as shown in the figure. $A + B$ and $A_1 + B_1$ were made equal, and were each about one-eighth of the resistance of the battery (E or E_1), plus that of the line and of the other station's apparatus. The adjustment was effected as in fig. 2, except that the tongue of G was also made to rest against the contact stop when no current was passing. This arrangement worked well on both artificial and actual lines, and is superior to the first.

Its action is as follows: For the sake of illustration let $A + B = G = 620$ ohms, $A_1 + B_1 = G_1 = 620$, $E = E_1 = 100$, and $L = 2590$. When the keys are at rest a current circulates in the system, the strength of which is the added effect of E and E_1,—viz. $\frac{1+1}{3410}$, 3410 being the sum of all the resistance in the circuit, and $1 + 1$ the sum of the electromotive forces. Of this only one-half or $\frac{1}{3410}$ goes through the relays, and, as we have seen, just suffices to keep them well open. Let K be held down, calculation and observation alike show that the current in G is not affected thereby;

E_1 still sends a current to line, which is practically the same as before, though in reality a little stronger $\left(\frac{1}{3309}\right)$, the slight increase, which we need not stop to appreciate, being due to the total resistance in circuit being now less than before the depression of K. But the greater part of the current from E now goes to earth *viâ* A and K and G B and K, and only $\frac{1}{40920}$ goes to line, or one-twelfth of its original strength. The current in the line is therefore $\frac{1}{3410}+\frac{1}{40920}$ or $\frac{1}{3148}$, and since but one-half of this traverses G_1 $\frac{1}{6296}$ instead of $\frac{1}{3410}$ represents the current by which the distant relay is actuated, and this by reason of the adjustment is insufficient to keep the tongue from falling over to the contact stop; a signal is therefore recorded. The same reasoning applies when K_1 is held down, G_1 is unaffected, while the current in G is reduced to nearly one-half, and permits the tongue to fall over to the contact stop, and record a signal. When both keys are down at the same time the relays receive a current which is almost the same as when they are worked singly, while a current of only $\frac{1}{20460}$ goes to the line. According, therefore, to the positions of the levers the line is traversed by currents of the following strengths: $\frac{1}{1705}$ when both levers are at rest, $\frac{1}{3148}$ when one up and one down, and $\frac{1}{20460}$ when both down. One advantage of this arrangement is that as it dispenses with the artificial line required by other systems it also dispenses with the use of condensers.

The system has been in successful operation on the Shiraz-Teheran line, Persia, 600 miles long, No. 5 wire. Any two stations possessed of a rheostat can easily join up duplex with the ordinary apparatus, and if they possess slides so much the better.

ON THE CONSTRUCTION OF CABLE-KEYS.

It very seldom happens that instrument-makers turn out a cable-key ready to meet the practical requirements of operators. No doubt this arises from the want of knowing what is really required. The most beautiful workmanship is often spoilt by some error in the arrangement of the parts of the key, and I will here endeavour to point out the errors which are usually made in the construction of keys for cable-working, and the remedies which can be applied in future.

Different people have different opinions, and doubtless there will be some who will differ from me even in the construction of a key, but I think I may claim as great an experience in the matter of cable-keys and cable-operators as any one, and the remarks I am about to make are the result of upwards of ten years' careful observation of persons and apparatus; and although there may be exceptions to every rule the following is the average result of my experience.

The errors in construction are—

I. The springs in the ordinary form of cable-key are too stiff, and require an amount of pressure to insure good contacts, which soon tires an operator's fingers, and causes him to send irregularly, and in a short time to considerably reduce his speed of sending.

I have now before me a new key, and it will perhaps be scarcely credited when I state that to bring the "dot" key down to the under contact requires a weight of 15 ounces, and to bring down the "dash" key it requires 17 ounces. Now imagine an operator working at the rate of only twelve words a minute, five letters per word, and three signals per letter, for one hour, with his fingers only, each signal being produced by a finger-pressure of one pound weight, plus a little for firm contact, and the result surprises you, for $12 \times 5 \times 3 \times 60 = 10{,}800$ lbs. per hour; multiply this by eight hours for a day's work, and it will be seen what an enormous amount of muscular power is exerted, in the aggregate, in a day's duty at the "light" occupation of cable work. It would be very little short of "cruelty to animals" to force a man to work such a key for a whole duty, setting aside the fact that such a key must

necessarily reduce the speed of sending after a very short time, in addition to the contacts being made occasionally too light, and causing loss of a dot here and a dash there, resulting in errors and repetitions.

No key should ever be sent out if the springs cannot be depressed upon the bottom contacts by a weight of eight ounces without springing. I have found this strength of spring most in favour with good cable operators, and such a key can be worked without fatigue.

II. The platinum contact-points in some keys are inserted in brass sockets and filed off flush; the consequence is that, as soon as the surface of the platinum is worn off, the contact is made as much upon brass as upon platinum, and failure of signals is the result. The platinum contacts should stand out well above the sockets, be of good size (say No. 10 gauge), and meet fairly.

III. The bridge is almost invariably placed too near the tappers, which is a great mistake. Although English operators, either at home or abroad, do not, as a rule, adopt the Chinese fashion in the matter of finger-nails, they do, as a rule, object to have a finger-nail of moderate length broken or bent back by contact with the sharp edge of the brass bridge, with even an eight-ounce pressure, every few minutes, owing to the cramped-up construction of the keys provided; such "accidents" lead to imperfect keying, occasional light contacts and failure of signals, and consequently to error.

In the keys, as supplied, there is not the diameter of an ordinary lead pencil between the back edge of the tappers and the bridge, while behind the bridge there is plenty of spare space, a little of which might with advantage be "brought forward." Scarcely a single operator works upon the *front* edge of the tappers. There are some who work upon the centres of them; but the great majority work upon the NE. quadrant of the dot key and the NW. quadrant of the dash key, calling the nearest edge of the key S. The manner in which the tappers become worn down after long usage is a proof of this observation.

To remedy the defect here pointed out there should be left, between the bridge and the back of the tappers, a clear space of from three-quarters of an inch to one inch. The comfort of working with

this arrangement during an eight hours' spell of sending, as compared with an equal spell of working with a key of the ordinary construction, can only be estimated by those who have to manipulate it.

IV. The shape of the tappers is, as a rule, wrong; the edges are too thin and sharp, and they by no means find favour with operators; the upper surfaces are, moreover, too concave. The tappers should be made but very slightly concave on the top; the edges should be made thicker, and rounded or bevelled off, and the diameter found to be most convenient is that of a bronze halfpenny.

It may be thought by some persons that the keys might have convex knobs like the land-line keys; but this would not do. In the Morse key there is but one lever worked by one hand, and the knob is usually grasped in the hand, or inclosed by the fingers, whereas in the cable-keys there are two levers, or springs, worked of necessity by the fingers of one hand, and slightly concave tappers are found most convenient for the fingers in sending. Upon a convex surface there would always be a liability of the fingers to slip off in sending, and operators therefore prefer the slightly concave tappers with an 8 oz. spring.

It must be borne in mind that the hand is not shifted from key to key in sending, but the wrist is stationary, resting upon the table or upon a raised block a little above the level of the table, and frequently upon a message-pad, so as to give more freedom to the fingers. When the springs are too strong (such as the 1 lb. springs above referred to), and in very quick sending, the wrist cannot be at rest, and then it is raised, bringing the assistance of the arm into play to provide for the extra power required, the fingers alone being insufficient for the purpose.

V. The distance from the centre of one tapper to the centre of the other should be about one inch and three-quarters.

By introducing these little improvements in the construction of the "ordinary cable-keys," Messrs. "Smith, Jones, Brown and Robinson" will confer some comfort upon cable operators, and receive their good wishes in return.

JAS. GRAVES.

Valentia, November 9, 1876.

QUADRANT ELECTROMETER WITH BIFILAR SUSPENSION.*

During the last nine months we have been regularly using a Thomson's Quadrant Electrometer, that arrived in this country a year ago. In May of this year we first observed that when a constant difference of potentials was measured the primary deflection D slowly increased to a limit $D + \delta$ in about ten minutes, δ being a small increase. When the quadrants were suddenly connected the zero was δ in advance of its primary position, to which it only returned after some time. δ is always of the same sign as D. This action has slowly increased as the weather has become hotter, and now (August) $\frac{\delta}{D}$ is as much as one-fifth if the needle be kept deflected for about one hour. When it was first observed, the instrument was taken to pieces and thoroughly cleaned, to avoid this abnormal action being complicated by shreds causing any loss from the needle to the quadrants. After charging the Leyden jar, an irregular wandering of the zero was observed when all the quadrants were kept connected. This may have been due partly from the air about the silk fibres drying, and partly from the loss of charge of the jar, being apparently (great at first) due to soaking in; for we had seen that at any time if the charge in the jar were suddenly altered the electric zero altered, even although it previously coincided with the mechanical zero, that is, the zero of the instrument when the quadrants, the inside and outside of the jar, are at the same potential. After this irregular motion of the zero had to a great extent subsided the previous "yielding" effect was again manifested. At first sight it appeared like an increase of difference of potentials between the quadrants, due to some electromotive force acting through a great resistance, and as in June we observed that the insulating paraffin-wax on the ebonite of the electrodes had slightly melted, and had spread itself as a thin layer over the upper surface of the quadrants, we hoped that we had discovered the cause of the phenomenon in this paraffin having inserted itself between the feet of the electrodes and the quadrants.

* A paper on Unifilar Suspension by the same authors will appear in a future number.

However, taking the electrometer to pieces and carefully cleaning the quadrants and the electrodes produced no improvement, and a direct measurement with a Wheatstone's bridge showed the absence of any resistance between the quadrants and any of their connections. Quantitive experiments prove that for a given charge in the jar, and for a given position of the quadrants, the deflection corresponding with any fixed difference of potentials is constant, and that the marked steady motion of the deflected needle is really due to a corresponding motion of the zero.

The phenomenon, therefore, bears the appearance of a viscous yielding or increase of the silk fibres under continued stress; still, as we are not aware of any such yielding being observed in any other quadrant electrometer, and as we have not experienced it ourselves in any of the electrometers that we have used in England, we put forward this suggestion with diffidence, and are anxious to elicit the views of the Members of the Society of Telegraph Engineers on this subject.

In bifilar suspensions a deflection of the needle ncreases the tension of the fibres, consequently, if this increased tension be maintained for a time, there will be probably a lengthening of the fibres. If, therefore, the needle and fibres were perfectly symmetrical, the effect of this would be to slightly lower the needle, and, if we neglect for a moment any alteration of electric attraction produced by this lowering of the needle relatively to the quadrant, this lowering would not alter the zero position. But if the original tensions of the fibres are slightly unequal, or if either of the fibres tends to yield more than the other for the same stress, then the needle, in addition to being lowered, will remain slightly deflected after the removal of the deflecting couple. And now, in addition to this, the lowering of the needle may probably, even if the yielding of the fibres were exactly equal, cause an alteration of the zero, from change of the electric attractions produced by the approach of the needle to the lower half of the quadrants due to want of perfect symmetry of the needle and quadrants.

The effect of inequality of tension we have verified to a certain extent by a direct experiment on a silk bifilar suspension, in connection with which there were no electric attractions; consequently,

we have spent much time in endeavouring to equalize the tensions in the fibres of the electrometer, by finding the position in which the couple-resisting deflection is a maximum, in accordance with the method recommended to be used with electrometers; such equalization, however, can be only approximate. For the present, therefore, we are led to regard this "yielding" phenomenon as due to an increase in the elongation of fibres, which, under the continued action of longitudinal stress, seems to be noticeable during the heat of a Japanese summer; combined with

1. Probable want of perfect equalization of tension in the fibres.
2. Want of symmetry of the quadrants.
3. The aluminium needle being possibly very slightly bent.

This fault in the action of the electrometer adds greatly to the labour of taking time observations, and yet if the above hypothesis is correct it is difficult to see how to find a remedy. It may be possible to exactly equalise the tension of the fibres, but the quadrants must remain unsymmetrical, since they are not exactly equal in area, so that they cannot be adjusted to be equi-distant, and at the same time so that the opening in the centre is a circle.

We find that $\frac{\delta}{D}$ is nearly constant for different deflections with the same position of the quadrants, but it increases in value as the quadrants are moved out, when, as is known, the sensibility of the instrument (the charge in the jar being constant) is also increased.

If δ be measured after one hour, then, when the quadrants are nearly touching, $\frac{\delta}{D}$ equals $\frac{1}{12}$, and when the quadrants are at their usual position $\frac{\delta}{D}$ equals $\frac{1}{5}$.

When all the quadrants are connected together and the electrometer is slightly tilted, the zero instantly suffers a great change, but if the instrument be left tilted the zero moves back towards the position it had before tilting, this return motion of the zero being rapid at first and very slow towards the end.

W. E. Ayrton.
John Perry.

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Tokei, Japan.

ON A NEW MODE OF STUDYING EARTH CURRENTS AND THE VARIATIONS IN TERRESTRIAL MAGNETISM.

A very small magnet with attached mirror is suspended by a single fibre of silk. Exactly underneath this magnet, but about four inches below it, is suspended by a few fibres, a much larger magnet, with copper dampers applied in the usual way.

Things being thus arranged, it is evident that the small magnet is nearly in an astatic state as regards the earth's directive force, whereas the large magnet is scarcely affected. If now the daily variations of the suspended magnet are due to changes in the directive force of the earth as a whole, in other words changes in the direction of the terrestrial lines of force, both magnets will be equally affected by such variations, consequently the variations of the small magnet will be no greater in consequence of its astatic condition than they would be if the lower magnet were removed. If, on the contrary, the daily variations are due, not to changes in the direction of the terrestrial lines of force, but to the action of local causes, such as earth currents, we shall have the daily variations largely magnified. I had an instrument of the form described above constructed about two years ago, and for a considerable time had half-hourly readings taken, simultaneously, of the position of the small magnet of the earth currents in a line running due north and south, and of the position of a magnet in a magnetometer constructed on Gauss's principle. I found,

1st, That the variations of the small magnet of the astatic magnetometer were greatly amplified;

2nd, That the variations agreed entirely neither with those of the earth currents nor with those of the ordinary magnetometer;

3rd, They were generally much more regular than the variations of the earth currents.

After I had been sometime at work with this instrument I found that a method of observing of a somewhat similar nature had been practised many years ago by Messrs. Barlow and Christie; but the method they used differed in this important point from mine: in

theirs the large magnet was fixed, whereas in mine it is freely suspended. When the magnet is fixed the slightest change in the direction of the lines of force of the earth will make a large alteration in the position of the small magnet, just as the slightest alteration in the position of the large magnet, relatively to the lines of force of the earth, will cause a large angular displacement of the small one. We are not able therefore by using a fixed magnet,

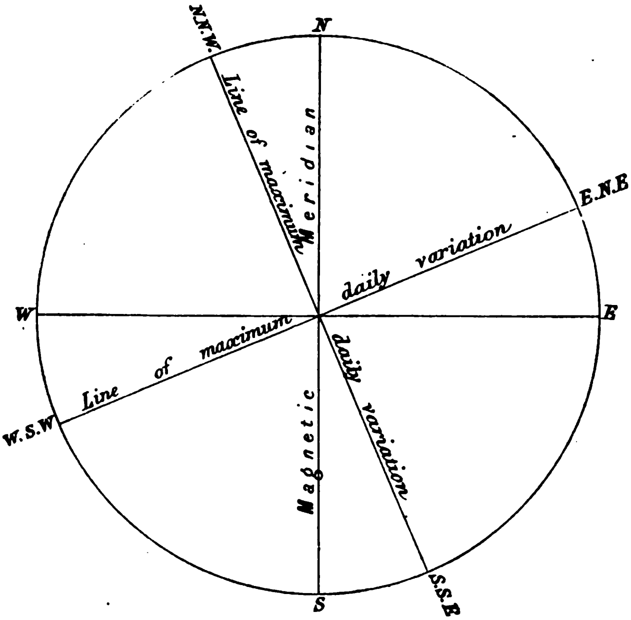


Fig. 1.

after the method of Barlow and Christie, to separate the effect of local actions from that of the variations in the directive force of the earth; nevertheless there is an important result to be gathered from their observations, which may not be generally known.

These able observers were not content with observing the variations of the astatic needle in the position of the magnetic meridian, but observed them also in a number of other positions, the magnet being deflected from its north and south position by a third magnet. They thus found that the amplitude of the variations differed in different azimuths, and they further determined that the position

in which the amplitude was greatest was that shown in the following diagram, which is copied from Barlow's paper on Magnetism in the Encyclopædia Metropolitana.

If now this diagram be compared with the following, which is copied from C. V. Walker's diagram, showing the direction of maximum earth currents (Philosophical Transactions for the year 1861, vol. 151, part I.), it will be seen that the position of maximum

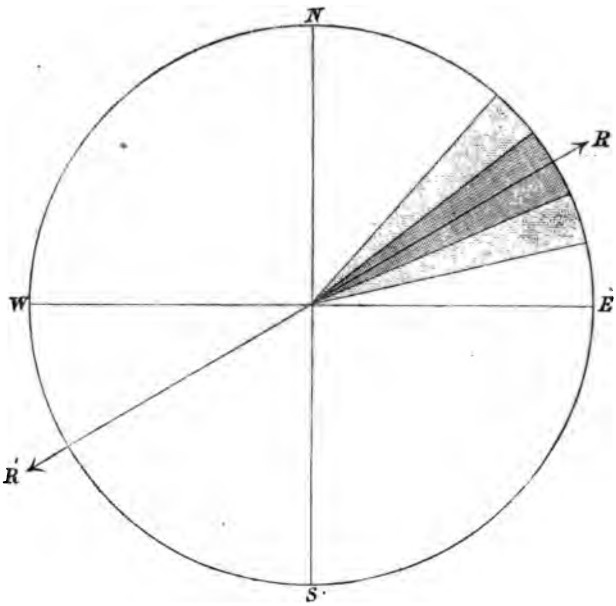


Fig. 2.

variation of the astatic needle, as determined by Barlow and Christie, corresponds very nearly with the direction of maximum earth currents as determined by Walker, years later, and as represented by the line R R′ in his figure.

DESCRIPTION OF SIR CHARLES WHEATSTONE'S AUTOMATIC INSTRUMENT.

By Captain V. Hoskiœr, Royal Danish Engineers.

[*Submitted to and approved of by the late Sir Charles Wheatstone previous to his demise.*]

I.—Description of the Instrument.

The paper ribbon is prepared by an instrument called the perforator or puncher with three levers, of which that to the left-hand side produces three holes (⁝) at right angles to the paper-ribbon, for a dot; the right-hand side lever produces four holes (°∞°) diagonally, for a dash; and the middle lever produces a small hole (∘) in the centre of the paper-ribbon, for spaces between the letters. The holes in the centre of the ribbon form a kind of rack, by means of which the ribbon is carried forward, that is to say, the teeth of a revolving star-wheel penetrate into the rack-holes. The dot and dash levers act also upon the centre lever, which produces the middle row or rack. The lever keys are struck with brass or iron punching sticks, furnished at the lower end with pieces of india-rubber, or they are worked by compressed air; in the latter case the punching is performed by means of three keys, whose touch is as light as those of a pianoforte.

The transmitter is driven by clockwork with a weight, and its speed can be adjusted between 12 and 120 words per minute. The perforated ribbon is carried forward by a small star-wheel, driven by the same clockwork. The perforated ribbon is pressed against the star-wheel by means of a friction-roller with grooves, which are meant to permit the needles to pass freely through the dot and dash holes of the ribbon, and to enable the teeth of the star-wheel to penetrate sufficiently into the rack-holes.

On the ebonite rocking beam V_1, kept in motion by the clockwork, there are two contact studs *a* and *b*, of which *a* is in connection with earth, and *b* with the disc D. Under the studs lie the levers A and B, pivoted independently, but in metallic connection with each other through the spiral springs F and the frame of the in-

strument, and pressed upwards against the contacts by mea
the spiral springs.

To the end of the levers A and B, and following their up-
down movements, are attached the needles M and S, of whic
regulates the marking current and S the spacing current.

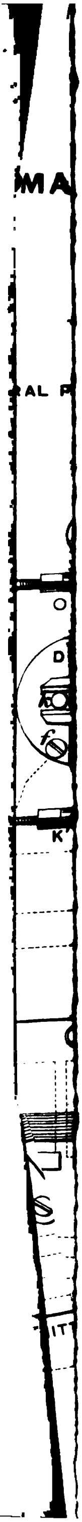
These needles are not fixed to any of the moving parts of
instrument, but they are simply forced upwards by the pressur
the spiral springs upon the levers A and B, and these levers rem
in contact with the contact-studs so long as the needles are p
mitted to move freely up and down.

The needles M and S thus determine whether the connecti
between battery and line shall be direct and with full power or no
They rise alternately, until they touch the perforated ribbon; if th
needle enters a hole, the current goes to earth with full strength
but if it does not hit upon a hole its motion and that of the leve
is stopped by the paper, and, as the stud on the rocking-beam continues to rise, contact is broken between the stud and the lever, and when this takes place a resistance R is inserted between battery and earth, considerably weakening the current and its effect.

As the rocking-beam and the levers oscillate, the disc D is pushed from one side to the other, and the small roller E, which is fixed on a spring, jerks it over after it has been pushed over the centre. Two pins prevent the disc from being pushed any further to the sides than needed. The disc D consists of an ebonite piece with a semi-circular metal piece on each side. On each half is a metal stud *k* and a terminal *f*, through which copper or zinc is put in connection with earth or line. The small ebonite piece *e* prevents the lever Z from coming into contact with the lever K, and thus putting the battery on short circuit.

In fig. 1 zinc is in connection with line and copper with earth, in fig. 3 copper is to line and zinc to earth.

When the instrument is in the position shown in figs. 1 and 3, the current flows through the lever B, the springs F, the frame of the instrument, and the lever A, direct to earth; but when the instrument is in the reverse position, where one of the needles has been opposed in its rising motion by the paper ribbon, and conse-
-ently the contact between the corresponding lever and stud on



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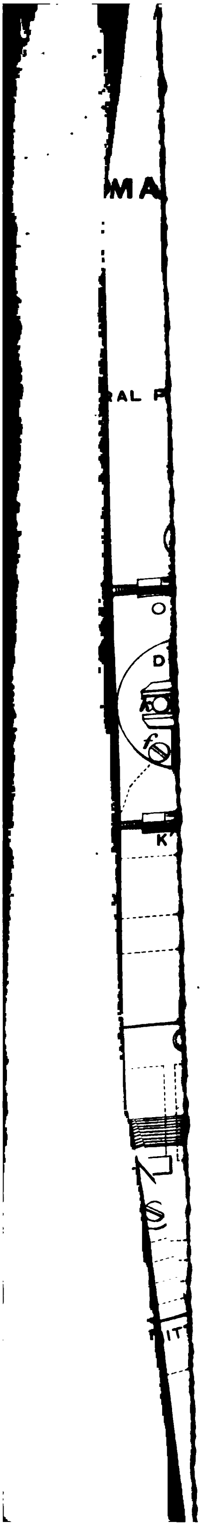

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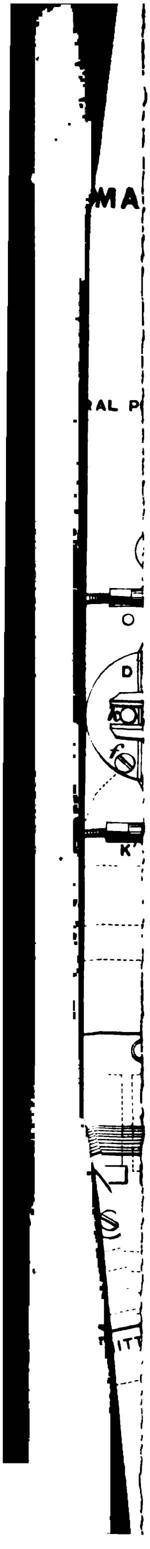

D
K

strument, and pressed upwards against the contacts by means of the spiral springs.

To the end of the levers A and B, and following their up-and-down movements, are attached the needles M and S, of which M regulates the marking current and S the spacing current.

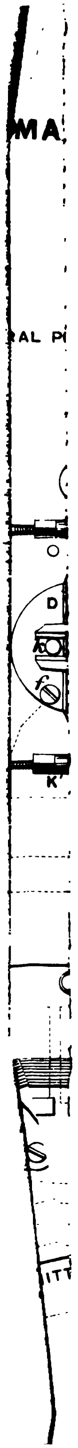
These needles are not fixed to any of the moving parts of the instrument, but they are simply forced upwards by the pressure of the spiral springs upon the levers A and B, and these levers remain in contact with the contact-studs so long as the needles are permitted to move freely up and down.

The needles M and S thus determine whether the connection between battery and line shall be direct and with full power or not. They rise alternately, until they touch the perforated ribbon; if the needle enters a hole, the current goes to earth with full strength, but if it does not hit upon a hole its motion and that of the lever is stopped by the paper, and, as the stud on the rocking-beam continues to rise, contact is broken between the stud and the lever, and when this takes place a resistance R is inserted between battery and earth, considerably weakening the current and its effect.

As the rocking-beam and the levers oscillate, the disc D is pushed from one side to the other, and the small roller E, which is fixed on a spring, jerks it over after it has been pushed over the centre. Two pins prevent the disc from being pushed any further to the sides than needed. The disc D consists of an ebonite piece with a semi-circular metal piece on each side. On each half is a metal stud *k* and a terminal *f*, through which copper or zinc is put in connection with earth or line. The small ebonite piece *e* prevents the lever Z from coming into contact with the lever K, and thus putting the battery on short circuit.

In fig. 1 zinc is in connection with line and copper with earth, in fig. 3 copper is to line and zinc to earth.

When the instrument is in the position shown in figs. 1 and 3, the current flows through the lever B, the springs F, the frame of the instrument, and the lever A, direct to earth; but when the instrument is in the reverse position, where one of the needles has been opposed in its rising motion by the paper ribbon, and consequently the contact between the corresponding lever and stud on


MA
D
f
K'

the rocking-beam broken, the current must go through the resistance R to earth.

The *receiver*, the speed of which can be adjusted between 12 and 120 words per minute, like the transmitter, is represented in fig. 2. When the switch S′ is in the position shown in the diagram, the received current passes through the bell; but when the switch is put upon the contact C the coils of the electro-magnets are in circuit and a small writing disc is set in motion. The clock-work of the instrument, driven by a strong spring, acts upon a larger disc with a groove, which dips into a reservoir of ink, and its groove being supplied by capillary attraction, the smaller disc takes its supply from the larger one without any friction against it.

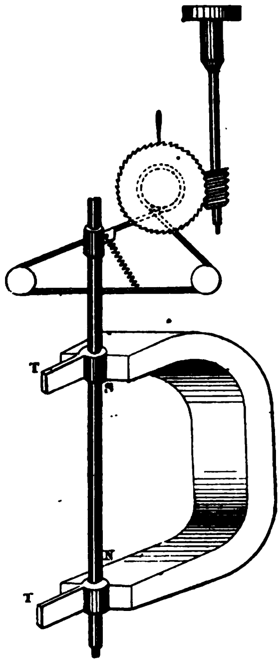


Fig. 4.

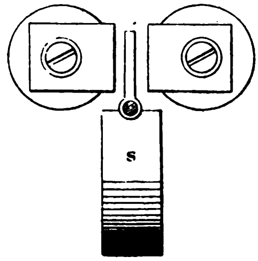


Fig. 5.

The current to the down line passes through the galvanometer, and the deflection on it shows if there is good connection with the cable.

The receiver can be regulated for a stronger or weaker current by means of the contrivance shown in figs. 4 and 5. Fig. 6 is a

perforated ribbon with the letters A, B, and C ready for transmission.

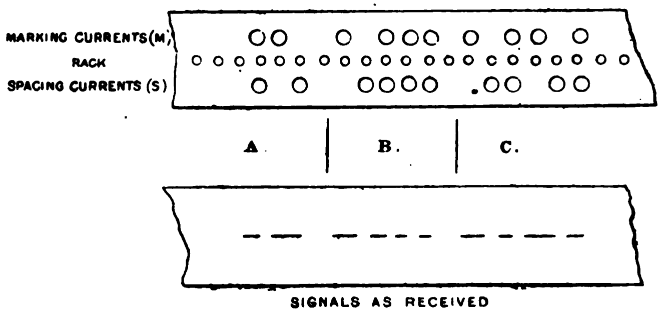


Fig. 6.

In fig. 7 is shown a perforated ribbon with the letters A, I, and T, and if these letters are sent the currents will flow as follows:—

First, the marking-needle M passes through the hole 1 in the perforated ribbon, putting zinc to line and copper to earth, as in fig. 1. This current acts upon the armature of the receiver in a direction to press the writing disc against the Morse slip, and the dot has been commenced. Now the spacing-needle S rises through the hole 2 in the perforated ribbon, putting copper to line and zinc to earth as in fig. 3, and this reversed current acts upon the armature so as to take the writing disc away from the Morse slip, and the dot is finished.

To commence the dash, the marking-needle again rises through the hole 3, putting zinc to line and copper to earth as in fig. 1.

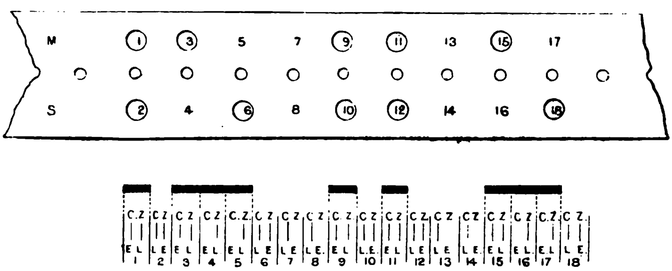


Fig. 7.

The spacing-needle now rises, but as it does not hit upon a hole in the paper at 4 (fig. 7) the rod H does not push the dice D over,

and as the rocking-beam V continues its rocking motion the position will be such as to place zinc to line and copper, through the resistance R, to earth, and consequently a current of less strength passes to line. Again the marking-needle rises, but as it is opposed by the paper at 5 (fig. 7) the disc D is not pushed over and the direction of the current is not altered.

At last when the spacing-needle rises through the hole 6, putting copper to line and zinc to earth, as in fig. 3, the dash is finished.

The marking-needle now rises without touching upon a hole at 7, the disc D is not pushed over, and, as the rocking-beam V continues its motion, the position will be such as to place copper to line, and zinc, through the resistance, to earth.

II.—How to clean the instrument.

Every morning the clerk in charge of the instrument should subject it to a thorough outer cleaning, by means of duster, brush, and washleather. The contacts should be cleaned by an experienced hand only. Paper dust in the slit under the star-wheel and round the top of the needles must be removed with a fine brush.

To clean the inside of the instrument it must be taken to pieces, but previous to this the proper position of all different parts of the instrument should be very carefully noted.

All steel parts are to be cleaned with washleather. All pivot holes to be cleaned with hardwood and then oiled. All other parts, subject to friction, should be oiled as well. If there is any brass-coating on the pivots it should be carefully removed with a little emery paper.

Wheels, axles, &c., are cleaned with brush, soap, and cold water, then dipped into dilute spirits of wine, rubbed with fine dry sawdust, and finally brushed with a dry, clean brush; or still better, brush the parts in dilute spirits of wine, clean the teeth with card-board, and finally brighten with chalk, brush, and wash-leather.

The contacts are cleaned as follows:—Dust them with a soft brush and observe that no hairs of the brush are left between or about the contacts, then scrape lightly with a blunt pen-knife. It should be remembered that the contacts must never be scraped

with a sharp instrument, as this would soon wear away the platinum.

The pen-knife, or whatever else may be used, should at the same time act as a scraper and a burnisher.

When the transmitter is to be taken to pieces, first remove from the front side of the instrument

1. One end of each of the spiral springs which press the levers against the studs of the rocking-beam.
2. The small ring, which is fixed upon the main axle.
3. The pressing-down roller.
4. The star-wheel, and
5. The stopper.

When this is done, place the instrument on its back, take out the four main screws, lift the front wall, but with great care, and, if necessary, loosen the two screws which fasten the instrument upon the mahogany base.

When everything has been well cleaned, and it is ascertained that the switch underneath the instrument is in proper working order, put all parts in their right place, put on the front side, and with care guide every pivot into its corresponding hole.

When a *rcceiver* is to be taken to pieces, first take off the ink-reservoir, and all working parts on the outside of the front; as the working disc and the grooved wheel are not fixed with screws on their axles, they are easily removed by means of a pair of plyers. When this is done, place the instrument on its back, loosen the wires to and from the electro-magnets, and finally take out and clean the different parts.

The construction of the puncher, the bell, and the key is so simple, and they are so easily taken to pieces and put together again, that any particular advice on this behalf must be considered unnecessary. Carefulness and patience only are required.

III.—How to work, repair, and regulate the instrument.

A.—*The Puncher.*

The punching clerk should work with a light and springy stroke. It is often found that, when one clerk considers a puncher quite

workable, another declares it to be useless, and the only reason for this difference of opinion is that the first clerk is a more experienced and more clever worker than the latter.

The punching clerk should, without fail, check the marks he produces on the paper ribbon, and immediately correct any mistakes he detects. This checking must go on together with the punching.

The punching clerk should ascertain that the wheel carrying the paper-roll inside the deck of the perforator moves round easily, that the cover of the box does not touch the paper roll, and that the ribbon passes unopposed to the front of the puncher. In cutting the paper into discs the edges are sometimes turned over, which causes the uncoiling to be difficult. To remedy this, take the paper disc and push its middle part out of place each way, then flatten it again.

If it be found that the paper slip does not advance properly in punching put more strain upon the back spring by means of the two fixing screws. It must be observed that the spring, which through the grooved steel wheel presses the jerking lever against the star-wheel, is properly regulated; if it presses too lightly, it weakens the force of the mainspring, so that this cannot move the star-wheel and the paper forward; if not tight enough, the jerking lever is pushed forward without moving the star-wheel, and consequently the ribbon would stop as well.

When the distance between the holes in the upper or lower row on the ribbon is uneven, regulate the stud affixed at the left-hand side of the punchers (the pins). The stud itself is firm, but the brass piece which holds it is movable.

When the ribbon is jerked forward one space only, instead of two, after the dash lever is struck, shorten slightly (by bending) the rod, which connects the dash lever with the crooked stopping lever.

When the ribbon is jerked forward two spaces instead of one, only by striking the dot lever or the space lever, the connecting rod should be stretched a little.

In this case, however, the fault might be due to a rather tight pivoting of the crooked lever, or the small screw which joins the connecting rod to the crooked lever may be out of order by being rusty, too tight, or the like. This of course should be attended to as well.

B.—*The Transmitter.*

This instrument is the most delicate and complicated part of Wheatstone's automatic apparatus, and should therefore be treated with the utmost care.

The winding-up should not be done in sudden jerks, as this would endanger the chain.

The stopper should not be turned abruptly, or struck from one side to the other, but gently put in the position required.

When the instrument does not start running immediately after the switch has been put on "send," it should not be forced to do so. A gentle turn or two of the pressing down wheel will generally have the desired effect, but, if not, the instrument should be examined to find out the cause.

The chain should not fall upon the mahogany base when the instrument is wound up.

All contacts should be cleaned every morning.

When a transmitter stops, the cause is generally one of the following:—

1. Dirt in the pivot-holes, &c. To clean this properly the instrument must be taken to pieces.
2. Oil, dirt, &c., on the friction discs. In this case the clockwork runs very unevenly, because the flywheel does not move along with the other parts. The friction discs should be cleared carefully with wash leather, which may be done without taking the instrument to pieces.
3. Want of oil, which causes the clockwork to run with a whizzing noise. In such a case apply a drop of oil to each of the small pivot-holes, which are found outside the instrument around most of the pivots and the bearings of the fly, and at the same time put the clockwork to run at its highest speed.
4. The axle of the pressing down wheel being dry or dirty, so that the wheel when lifted up from the ribbon by depressing the opposite end of the frame does not run freely for a good while, after once being started. Take off the wheel, clean

the pivots and the pivot-holes well, and apply a drop of oil, but be sure not to turn the wheel over, as each pivot seldom fits both holes.

5. Paper rubbish on or about the star-wheel, which prevents the needles from passing freely up and down. If the slits are quite choked with rubbish, the instruments will stop altogether, and this of course can soon be seen and remedied.

   It is more dangerous, however, if the slits are not quite filled up, as the clockwork then may still run, but the needles being slightly obstructed the levers will not be able to follow the movements of the rocking-beam, and consequently the contacts between battery and earth are uncertain, and the signals will be lost. After removing the top of the front part of the instrument, the star-wheel and the slits are easily cleaned.

6. The pivot of the fly-wheel axle having drilled a hole into the diamond (agate). The only remedy in this case is to put on another spring with a new diamond.

7. The chain broken. When putting the chain together again it must not be twisted. The connecting link should be annealed before bending it, and tempered again afterwards.

When any obstruction is felt in winding up the weight, the cause will be found to be one of the following:—

1. The stopping contrivance at the back of the instrument does not readily sink down as the weight goes down, and therefore it continues to stop. For a single time the stopper might be depressed by means of a knife or the like, but it is advisable to have the contrivance properly cleaned at once.

2. The main axle dry, dirty, and rusty. First remove the driving weight, take out the hole wheel, together with the axle, from behind the instrument, then force the axle out of the wheel, if needed by hammering; on that end of the axle where the winding-up handle is put on, but before using the hammer, all rings, screws, &c., fixed upon the axle must be taken off.

3. When winding up the weight the chain falls upon the instru-

ment's base, instead of going through the hole intended for it. The edge of the hole should be rounded off, so as to cause no obstruction, but at the same time it should be ascertained that the forked brass piece which is meant for taking the links off the cams is in its proper position.

The transmitter is regulated by means of—

1. The two screws against which the needles M and S are pressed.
2. The spring upon which the roller E is fixed.
3. The two insulating collets K, K′, are the rods H and H′, and
4. The screw above the ebonite piece *e*.

It is to be observed that the sideway movements of the disc D should resemble the writing with the Morse key, and be in accordance with the marks on the perforated ribbon.

When regulating the transmitter, put it in circuit, with a good receiver, and a few cells.

C.—*The Receiver.*

This instrument is not quite so complicated as the transmitter, but nevertheless it ought to be handled with care. The winding up should be done without jerking.

If the clockwork does not start running immediately after gently putting the stopper into the proper position, a slight pull at the paper slip generally has the desired effect.

It should be ascertained that the slip is drawn out of the box without any obstruction, and that the received marks are imprinted on the convex side of the slip, which is of particular importance when the received slip is pasted upon the message form. As the paper rolls are cut after being wound up, and consequently the cut starting at the outside, a slight bend will always be produced.

When the clockwork stops, the cause will be found to be similar to that which is pointed out for the transmitter. The clockwork may stop, because the oil around the spring (in the drum) has been used up or has become dirty, in which case let the spring run out, take off the front wall of the instrument, when the drum will readily come out, then unscrew the drum-cover and put on a fresh supply of oil.

When the clockwork cannot be wound up, the cause is generally something similar to what has been said about the transmitter, but of course with the difference occasioned by the receiver being driven by a spring, and the transmitter by a weight.

The receiver is regulated by means of the two small regulating screws, which are fixed close to the armature of the electro-magnets and the bottom of the instrument. When the regulation takes place, the receiver should be in circuit with a good transmitter and a few cells. It is to be observed that the more limited the play of the armature the more delicate is the instrument, but of course only up to a certain point. If the play of the armature is too little, the vibration of the moving clockwork will frequently cause a series of dots to be printed when no current is passing, and, while receiving, this causes the marks to run together.

D.—*The Bell.*

The bell should ring after four reversals from one cell have been brought to bear upon it. The regulation is performed by two regulating screws, as with the receiver.

E.—*The Keys.*

The keys can be divided into three chief groups, viz.:—

Keys with rubbing contacts,
" " spring " and
" " piston "

The first-named is that which at first was used in connection with Wheastone's automatic instrument, and is still considered the safest, particularly for cable work.

The key is easily maintained in order, as the only thing necessary is to see that the contacts are clean and touch at the proper time.

Spare Articles desirable at each Station for Wheatstone's Automatic Apparatus.

For the Perforator:—

½ dozen star-wheels.
4 India-rubber washers.
4 feet India-rubber for punching sticks.

For the Receiver :—

1 drum with spring and axle.
3 springs.
3 stopping contrivances (brake levers).
1 dozen springs and agates for the fly-wheel.
½ ,, small axles.
¼ ,, chains.
2 spirals for the fly-wheel.
3 writing discs.
3 larger discs with grooves.

For the Transmitter :—

½ dozen chains.
½ ,, star-wheels to move the perforated paper-slip.
2 spirals for the fly-wheel.
½ dozen small axles.
1 ,, springs with agates for the fly-wheel.
Platinum for Contacts.—Spiral Springs of different sizes.

For the Bell :—

2 sets of axles.
4 springs.
2 drums with axle.

Shiraz, Persia,
20 July, 1876.

To the Secretary,
Society of Telegraph Engineers.

SIR,—In examining the discordant results contained in Mr. Graves's paper "On the Internal Resistance of Batteries" (vol. ii. pp. 130—134), I find that they are due to an oversight in working out the data there given.

By substituting the formula $x = \frac{RS\,(g + s)}{g\,(s - S)}$ (s being the shunt used in reproducing the deflection), the apparent discrepancies will disappear.

For example, take the first line in each set, page 132, for shunts 1 and 3 ohms respectively:—

$$\frac{12000 \times 1 \times (5854 + 5{\cdot}86)}{5854 \times (5{\cdot}86 - 1)} = 2471;$$

$$\frac{2360 \times 3 \times (5854 + 5{\cdot}86)}{5854 \times (5{\cdot}86 - 3)} = 2478.$$

And again, page 133—

$$\frac{5600 \times 1 \times (5854 + 5{\cdot}86)}{5854 \times (5{\cdot}86 - 1)} = 1153;$$

$$\frac{1100 \times 3 \times (5854 + 5{\cdot}86)}{5854 \times (5{\cdot}86 - 3)} = 1155;$$

the results in each case, with a shunt of 1 and 3 ohms, being identical.

Yours faithfully,

J. J. Fahie.

To the Secretary, Valentia, October 25th, 1876.

Society of Telegraph Engineers.

Sir,—In reference to Mr. Fahie's communication, permit me to state that when I made those experiments in 1873 the discrepancies in the results were so striking that I was induced to send them to the Society in the hope that some one might be able to reconcile them.

After a lapse of upwards of three years Mr. Fahie has kindly supplied a formula which completely clears the ground, and reconciles apparent contradictions, and it is satisfactory to have thus proved that Nature's laws are true, however erroneously we are liable, at times, to interpret them.

Yours faithfully,

J. Graves.

The Eastern Telegraph Company Limited,

To the Secretary, May 1st, 1877.

Society of Telegraph Engineers.

Dear Sir,—Please allow me to draw your attention to a printer's error which appeared in the Society's Journal, No. XIII. and XIV.

vol. v. page 254, containing my paper on "A new method of taking the Loop Test." At foot of page 254 you will find—

$$\frac{r_1 R_2 + r_1 R_2}{2 R_1 R_2} = \frac{X}{Y} \therefore X = \frac{Y(r_1 R_2 + r_1 R_2)}{2 R_1 R_2},$$

instead of the following, which it should have been—

$$\frac{r_1 R_2 + r_2 R_1}{2 R_1 R_2} = \frac{X}{Y} \therefore X = \frac{Y r_1 R_2 + r_2 R_1}{2 R_1 R_2}$$

The mistake being in the small figures attached to r and R.

I should feel much obliged if you can draw attention to the above in your next number.

I am, dear Sir, yours faithfully,

ANDREW JAMIESON,
Electrician.

S.S Chiltern, Malta.

BELL'S TELEPHONE.

With the object of stimulating inquiry into the means of improving the telephone of Mr. Bell, which is the most beautiful adaptation of telegraphy ever made, I desire to draw attention to a few simple methods by which any one may satisfy himself of its practicability, for no one having witnessed its performance can fail to see a great future before it.

The recorder of Sir W. Thomson affords a ready means of speaking, and gives out such clear tones as to make the listener at first involuntarily look for the speaker behind the instrument (who may be miles away). It suffices to take a tube two inches in diameter, and stretch over one end a membrane of parchment or thin gutta-percha (the latter is less affected by the breath, the former becoming somewhat flaccid after a time). To the centre of the membrane cement a straw, and fix the tube in front of the instrument; about six inches from the "movable coil" cement the other end of the straw to the coil at the point where the silk fibre is

usually fixed; this is all that is necessary for both speaking and receiving. Six or eight cells on the electro-magnets suffice.

A pair of these tubes may also be connected in a similar manner with the tongues of two polarised relays. The tube is to be fixed in a convenient position at right angles to the tongue, and the free end of the straw cemented to the tongue, taking care that the latter is free of its ordinary contact points. No battery is required for speaking with this arrangement.

Or a pair of these speaking tubes may be connected with the ordinary armatures of any instrument or relay, and a current kept on the line. The armature should, however, not be too heavy, and should be carefully adjusted. The best adjustment gives the loudest sound. In sending, be careful the armature in vibrating does not touch the cores of the electro-magnet.

A plate of thin iron, such as is used for stove pipes, fixed to an upright board, the latter hollowed out on the side on which the plate is fastened, and a hole made in the board in front for inserting a convenient tube for speaking, may be used as an armature, and a pair of coils placed in front of the iron plate through which a current from a battery is flowing, the cores to be adjusted as close as possible to the plate; this answers for sending and receiving. The battery need not be strong; if it be so, the armatures have to be removed further away from the coils. On a short line the resistance of the coils with a suitable battery is of little importance. I have spoken as well with small coils of 3 ohms as with 400 ohms.

If a pair of coils at the receiving end be placed on a violin, and connected to the line on which there is a permanent current and a sending instrument as above, singing and speaking into the tube at the distant end can be heard by placing the ear to the violin. The effect is exalted by laying a plate of iron on the poles of the electro-magnet.

By these simple means—and they are selected as being within the reach of many—may be demonstrated the possibility of speaking over miles of telegraph line.

The sound of the voice in the tube is not that of a whisper but of

a voice at a distance—and the *nearer* you seem to bring the sound the better your adjustment, and *vice versâ.*

I have spoken through four knots of buried cable without sensible diminution of effect.

When the instruments are not well adjusted some words will come clear when others do not, and I have found the sentence "are you ready," pronounced deliberately, intelligible when others were not.

The object to be sought for is to augment the strength of the variations of current. At present it is limited by the power of the voice to move an armature or coil, and unless it can be magnified by putting in play a reserve of force, as compressed air, &c., improvement cannot go far.

The most hopeful field seems to be the effecting a variation through a sensible range of resistance at the sending end to vary the strength of current in a primary coil by shunting or varying the resistance of a battery circuit—as for example a fine wire inserted more or less in mercury.

JNO. GOTT.

St. Pierre, Miquelon,
March 24th, 1877.

ABSTRACTS AND EXTRACTS.

TRANSMISSION OF MUSICAL TONES BY ELECTRICITY.

Although much has been said and written of late on the subject of sound transmission by electricity and its application to telegraphy, there still remains much of interest to be said.

As sound transmission, electrically, is likely to play an important part in the telegraph of the future, it will be of interest and importance, especially to telegraphers, to have an intelligent understanding of the principles involved.

As far back as 1837 Page discovered that the magnetisation or demagnetisation of soft iron was accompanied with sound. About the same time Professor Henry studied the acoustic effects of a galvanic current upon an electro-magnet, and determined that it was due to the elongation and contraction of the iron, and not the attraction of one limb of a horseshoe magnet for the other, as at first supposed. He determined this by making a straight magnet produce the same effect as the horseshoe shape.

This discovery stimulated research among the scientific men of Europe for a few years, but without making any great advance in the science of telephony. Wertheim's apparatus for producing a simple tone in an iron rod by an automatic rheotone, and that of Reis, which transmitted tones differing in pitch by means of a diaphragm thrown in vibration by the voice, were, perhaps, the furthest advance in the way of apparatus in that line until within a very few years. They were used as mere scientific toys. No attempt, or at least no successful attempt, was made to adapt this

discovery to any of the useful arts or industries. In fact, further research and discovery were necessary before it could be so adapted.

Within the last few years a spirit of re-investigation and further discovery has started up, and, as in the first instance, it starts in our own country.

As above stated, before this principle of sound transmission could be applied economically and practically for telegraphic purposes, there remained much to be discovered and much to be invented, in the way of details of apparatus and methods of connecting the same to the line and batteries, as well as the discovery of more fundamental principles. All this was necessary before the present capacity of the system was attained—that of transmitting eight messages simultaneously through a single wire.

This result has been accomplished by Elisha Gray, of Chicago, an electrician of large experience as an inventor and constructor of electrical and telegraphic apparatus.

Propagation of a Sound Wave.

Every-one has watched the expanding ring produced by a pebble dropped in a smooth sheet of water. Now, imagine a globe in the air starting from a common centre and expanding uniformly in every direction at the rate of 1,100 feet per second, and you have a picture of the air when a single sound wave or pulse passes in the ordinary way. The wave is set going by some force exercised at the starting point, as, for instance, an explosion. The heated centre expands violently in every direction, forcing the first layer or shell of air particles against the second, which in turn delivers up its blow to the third, and so on to infinity. When the shell of particles next the tympanic membrane of the ear is reached it vibrates by the blow, and conveys to the brain the sensation of sound.

An aerial wave and an electrical are analagous in some respects and very different in others. As we have intimated before, iron elongates when magnetized and shortens when demagnetized. If, an iron rod, surrounded by a helix, is mounted upon a sound-board and magnetized by closing a battery through the helix, a distinct

sound will be heard. This is occasioned by a sudden change in the length of the iron communicating a blow to the sound-board, which, by reason of its large surface, is able to communicate its motion to the air, which conveys it to the ear, as before described.

Suppose a common electric magnet to be mounted, as shown in fig. 2, upon a suitable sounding apparatus, and connected in a telegraphic circuit—say at New York, while at the other end of the line—say at Boston—is placed a battery and a common telegraphic key. If the key is suddenly closed a single blow will be heard at New York. If the key is opened another blow will be heard; the former made by expansion, and the latter by contraction of the iron cores.

It will be observed that in this case the sound is not transmitted mechanically through the wire, but that a mechanical motion at the sending end has sent an electric impulse through the wire, which has been converted into a corresponding mechanical motion at the receiving end, and makes its impression upon the ear in the ordinary way.

A noise becomes musical when it is repeated uniformly at a definite rate per second—not less than sixteen, or it may be many thousand. The pitch of any musical tone is determined by the number of vibrations, or the number of periodic repetitions of a single sound-wave per second; the greater the number the higher the pitch.

Keeping these facts in view, it will be plain to the reader that, if the operator at Boston could open and close his key with sufficient uniformity and periodicity—say at the rate of one hundred closes per second—a musical tone would be heard in New York proceeding from the magnet mounted upon the sound-board.

Early in 1874 Mr. Gray constructed a variety of transmitters for sending tones of varying pitch; one of which is shown in fig. 1, and a detached section at fig. 1*a*. We give his own description:

"Each key has a steel reed or electrotome, tuned to correspond to its position in the musical scale. A better understanding of the operation of a key and its corresponding electrotome may be obtained by referring to the detached section shown in fig. 1*a*.

a is a steel reed tuned to vibrate at a definite rate corresponding

to its position in the scale. One end is rigidly fixed to the post *b*, while the other end is left free, and is actuated by a local battery. The magnets *e* and *f* are arranged in the same local circuit, magnet *f* having a resistance of about thirty ohms, and magnet *e* about four ohms. When the reed *a* is not in vibration the point G is in electrical contact with it, which throws a shunt wire entirely around the magnet *f*; thus, practically, the whole of the local current passes through magnet *e* at the instant of closing the key C. It is well known that when two electro-magnets are placed in the same

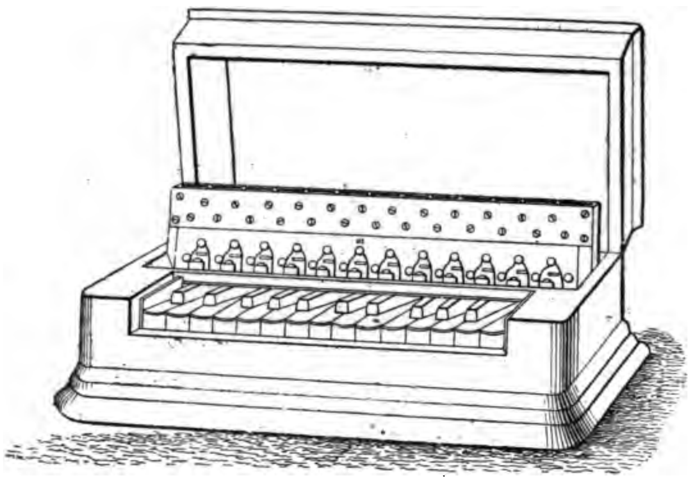


Fig. 1.

circuit the one which has the higher resistance (other things being equal) will develope the stronger magnetism, and that, if the magnet of higher resistance be taken out of the circuit, the force of the other will be increased. When the key C, being depressed, closes the local circuit at *d*, the operation of the reed is as follows: The whole of the current from battery L passes through the magnet *e*, which attracts the reed, say with a power of four. When the reed has moved towards I far enough to leave the point G, the shunt circuit is broken, and the current flows through both the magnets. Immediately the power in *f* rises from zero to five, and that of *e* drops from four to one, and the reed is attracted towards *f* with an

effective force of four, until contact is again established with the point G. The operation is repeated at a rate determined by the size and length of the reed, and which corresponds with the fundamental of the note it represents. The figures given above only approximate the facts. The relation of the magnets as to size and resistance, so as to give an equal impulse to the reed in both directions, was determined by actual experiment with a battery of a given size.

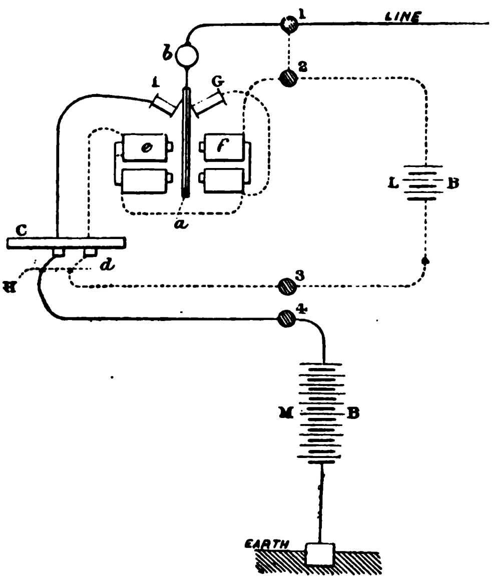


Fig. 1A.

It will be observed that by this arrangement the centre of vibration coincides with the centre of the reed when at rest, so that the pitch of the tone is not disturbed by any ordinary change of battery, as is liable to be the case when only one magnet is used, or when the impulse is not equal in both directions.

A second battery, which we will call the main battery, is connected as follows: One pole is connected to the ground; the other runs to the instrument, and, entering at binding-screw 4 (fig. 1a), runs to the point H of key C; from key C to point I, which makes contact with the reed a; from reed a to binding-

screw 1, and thence to line. It will be seen that when the key is at rest the batteries are open at the points *d* and H.

All the keys in the instrument, whether one or more octaves, have corresponding reeds and actuating magnets, the only difference being in the tuning of the reeds. There is but one main and one local battery used, and the connections to each key are run in branch circuits from the binding-screws, as shown in fig. 1*a*. But, since all these branches are open at the key points, neither of the batteries is closed unless a key is depressed.

If now the keys are manipulated, a tune may be played which is audible to the player. When any key is depressed, the local battery sets in vibration its corresponding reed, which sounds its own fundamental note according to the law of acoustics. So far the instrument is an electrical organ, the motive power being electricity instead of air. The main battery has had no part whatever in its operation.

If, however, the main circuit is closed by connecting the distant end to ground, and the point I is properly adjusted, so that it makes and breaks contact with the reed at each vibration, a series of electrical impulses, or waves, will be sent through the line, corresponding in number per second to the fundamental of the reed.

Now, as the pitch of any musical tone is determined by the number of vibrations per second made by the substance from which the sound proceeds, it is clear that if these electrical waves can be converted into audible vibrations at the distant end of the line, whether it be one mile or five hundred miles from the player, the note produced will be of the same pitch as that of the sending-reed.

One method of converting these "electrical waves" into audible sound at the receiving end has been described—that of the mounted electro-magnet. When it is properly mounted, so as to give the best acoustic effect, the tone is very loud, if the line is not too long or the battery too weak.

Fig. 2 shows one of Mr. Gray's musical receivers. It is a wooden box, with holes for acoustic effects, and having mounted upon it a common electro-magnet, with a heavy armature made

fast to its poles, and the whole screwed firmly to the box. Another of his musical receivers, not shown by cut, consists of a series of wooden boxes, open at one end, one for each nut or electrotome of the transmitting instrument. These boxes vary in size and are tuned to correspond to the key-board of the transmitting organ. They are ranged side by side, about an inch apart, and all firmly fastened to a wooden bar running across the whole. Upon this bar is mounted a magnet, like the one shown at fig. 2.

This arrangement retains all the sound-board qualities of the box, fig. 2, combined with reinforcement of each note by resonance, and consequently is much louder and of a different quality.

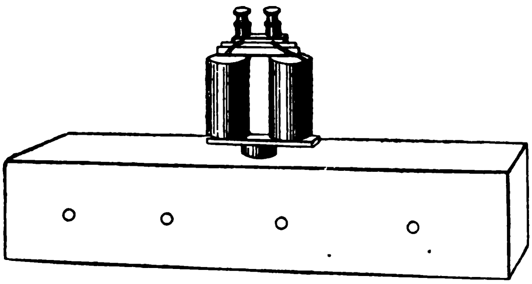


Fig. 2.

There are many methods of receiving musical tones, but perhaps the most curious is the one in which animal tissue plays a prominent part. If the operator places himself in the receiving end of the circuit, through which a tune is being sent, and, with his hand, rubs a metal plate that constitutes a part of the circuit, the tune that is playing at the other end of the line will be reproduced on the plate. This phenomenon was observed by Mr. Gray on the zinc lining of a bath-tub. He describes the circumstance as follows:—

"My nephew was playing with a small induction coil, and, as he expressed it, was 'taking shocks' for the amusement of the smaller children. He had connected one end of the secondary coil to the zinc lining of the bath-tub, which was dry at that time. Holding the other end of the coil in his left hand, he touched the lining of the tub with the right. In making contact his hand

would glide along the side for a short distance. At these times I noticed a sound proceeding from under his hand at the point of contact, which seemed to have the same pitch and quality as that of the vibrating electrotome, which was within hearing. I immediately took the electrode in my hand, and, repeating the operation, to my astonishment found that, by rubbing hard and rapidly, I could make a much louder sound than the electrotome was making. I then changed the pitch of the vibration, increasing the rapidity, and found that the pitch of the sound under my hand was also changed, it still agreeing with that of the vibration. I then moistened my hand and continued the rubbing, but no sound was produced so long as my hand remained wet; but as soon as the parts in contact became dry the sound reappeared. * * * * *

Fig. 3.

Following out the idea suggested by the bath-tub experiment, I constructed various devices with metallic plates for receiving the tune by rubbing with the hand. A very convenient method for doing this is shown in fig. 3.

This instrument has a metal stand of sufficient weight to keep it in position while being manipulated. Upon the stand a horizontal shaft is mounted in bearings, upon one end of which is a crank, with a handle made of some insulating substance. Upon the other end is centred a thin cylindrical sounding-box, made of wood, the

face of which is covered with a cap made of thin metal, spun into a convex form to give it firmness. This box has an opening in the centre to increase its sonorous qualities. The metal cap is electrically connected to the metal stand by means of a wire.

If the operator connects the cap, through the stand, to the ground, and taking hold of the end of the line with one hand presses the fingers against the cap, which he revolves by means of the crank with the other hand, the tune that is being played at the other end of the line becomes distinctly audible, and may be heard throughout a large audience room. If the conditions are all perfect, the faster the plate is revolved the louder will be the music, and the slower the motion the softer will it become. When the motion stops the sound entirely ceases.

I have found that electricity of considerable tension is needed to produce satisfactory results, at least that of fifty cells of battery. The necessary degree of tension is most conveniently obtained by passing the line current through the primary circuit (adapted to the circuit wherein it is used) of an induction coil and connecting the receiver in the secondary circuit. * * * * I noticed that when revolving the plate with my finger in contact the friction was greater when a note was sounding. I then connected a small Ruhmkorff coil to a battery, inserting a common telegraphic key in the primary circuit, instead of the self-acting circuit breaker. I connected one end of the secondary coil to the metal plate, and holding the other end in my hand I rubbed the plate briskly, and had my assistant slowly making dots with the key. I noticed at each make of the circuit a slight sound, and at each break a very much louder one, owing to the fact that the terminal secondary wave is much more intense than the initial. I now held my hand still, and, while I could feel the shock just as distinctly as before, there was no audible sound, proving that the motion was a necessary condition in its production. The sensation when the sound was produced was as though my finger had suddenly adhered to the plate and then as suddenly let go, producing a sound.

The next experiment was with one hundred cells of gravity battery. I connected one pole to the plate and held the other in my

hand, pressing my finger against the plate and revolving it as before. I inserted a thin piece of paper between my fingers and the plate to prevent painful effects from the current, and my assistant made dashes with a key in the circuit. I was thus able to notice the effect of an impulse of longer duration. When the key closed there was a perceptible increase of the friction, so that my finger took a position further forward on the plate, where it would remain as lông as the circuit remained closed. As soon as the key was opened my finger suddenly dropped back on the plate, making the same noise I had before heard. This operation was repeated so often that there could be no question as to the effect it produced.

From the foregoing experiments I find that the following conditions are necessary to reproduce musical tones through the medium of animal tissue by means of electric waves transmitted through a telegraph wire:

1st. The electrical impulses must have considerable tension in order to make the effect audible.

2nd. The substance used for rubbing the receiving plate must be soft and pliable, and must be a conductor of electricity up to the point of contact, and there a resistance must be interposed, very thin—neither too great nor too little.

3rd. The plate and the hand or other tissue must not only be in contact, but it must be a rubbing or gliding contact.

4th. The parts in contact must be dry, in order to preserve the necessary degree of resistance."

We have now to consider, first, the transmission of composite tones; and, secondly, the analysis of them, and their application to multiple telegraphy.

Referring to the cut (fig. 1) it will be seen that in this instrument there are a series of keys connected with a series of reeds, or electrotomes, tuned to the musical scale. If the batteries are properly connected to the instrument, and either of the tune-receivers described be placed in the line, not only will the tone of any *single* key but tones from a series of keys be reproduced simultaneously, should they be operated at the same time, whether the result of the combination be concordant or discordant. If all the keys corresponding to an octave of tones be depressed simultaneously, the

result will be a confused babel of sounds, with no defined tone. This same effect will be faithfully reproduced upon the common receiver, shown in fig. 2. The peculiarity of this receiver is, that it reproduces one note as well as another, and is incapable of rendering any one more prominent than its neighbour. How to sift this babel of sounds, and make a particular tone corresponding to a particular key respond, and respond only on a particular instrument—to explain this will be our chief object.

If we strike a series of piano keys at the same time, the ear will appreciate the effect as a whole and not as a series of simple tones. It is true an educated ear can select the simple tones, but, ordinarily, this is not the case. If, however, we place to the ear a resonator, such as Helmholtz used in analysing musical sounds transmitted through the air, the tone which corresponds in pitch to that of the resonator will be reinforced, so that it can be very distinctly heard above the others. This resonator depends upon the principle that a volume of air, contained in any open vessel, as a bottle for instance, yields a certain note when thrown in vibration. The pitch of this note depends upon the size of the vessel and its uncovered opening. The form used by Helmholtz was that of a globe, with a large opening on one side and a small one on the other, which is placed to the ear.

As we said before, if one of these resonance globes is held to the ear, and there exists in the air near by a series of musical tones, and one of them accords with the fundamental of the globe, that tone will be strengthened and distinctly audible above the others. With a series of these globes compound tones can be analysed and their simple elements determined with unerring accuracy.

A series of tones may be transmitted through a telegraphic wire and reproduced as a whole. In this shape they could not be utilised for telegraphic purposes; but if a series of receivers are placed in the line, as many as there are tones simultaneously transmitted, and each receiver has the ability to reproduce a particular note and reject all others, the problem is solved. This result Mr. Gray has accomplished in a variety of ways. We will mention a few of the more prominent. The first one is shown in fig. 4. We give his own description of the cut.

"Fig. 4 is a perspective of one form of a receiving instrument, called an analyser. The construction of the instrument is very simple. It consists of an electro-magnet, adapted to the resistance of the circuit where it is intended to be used, and of a steel ribbon strung in front of this magnet in a solid metal frame, and provided with a tuning screw at one end so as to readily give it the proper tension. The length and size of the ribbon depend upon the note we wish to receive upon it. If it is a high note, we make it thinner and shorter; if a low note, we make it thicker and longer. If this ribbon is tuned so that it will give a certain note when made to vibrate mechanically, and the note which corresponds to its fundamental is then transmitted through its magnet, it will respond and vibrate in unison with its transmitted note; but if another note be sent which varies at all from its fundamental it will not respond. If a composite tone is sent the ribbon will respond when its own note is being sent as a part of the composite tone, but as soon as its own tone is left out it will immediately stop. Thus I am able to select out and indicate when any note is being sent, in fact to *analyse* the tones which are passing over the line."

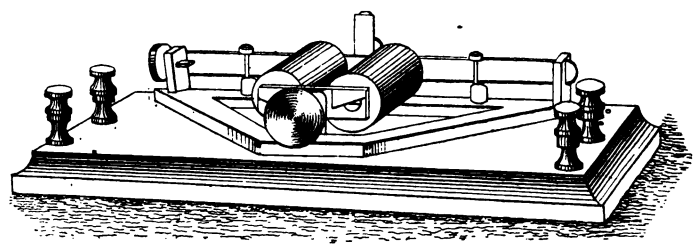


Fig. 4.

The great object to be attained in the construction of such a receiver is to rid it, as far as possible, of all sound-board qualities, which would cause it to respond alike for all tones, and *augment*, as far as possible, its tendency to vibrate for a certain tone only to which it is tuned. Another method is shown at fig. 5, which combines a tuned reed, or bar of steel, with a resonance box.

The construction is as follows: Suppose we wish to construct a receiver to respond to a tone made by 128 vibrations per second. We first make a box in the form of a parallelogram, as shown in

the cut, with one open end. The box is made the right size to produce a maximum resonance of the desired tone. Upon this box is mounted firmly an electro-magnet. To one pole is fastened one

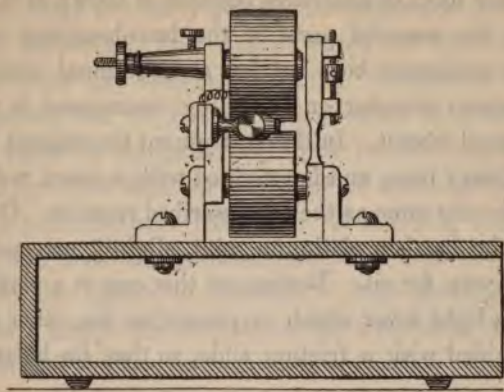


Fig. 5.

end of a steel bar having parallel sides, which extends across the other pole, but does not touch it. This pole, for convenience, is made adjustable. The tuning is all done at one point, near the fixed end of the bar, by filing away until it will vibrate 128 times per second, if excited mechanically. When thus tuned and mounted, if by any means the bar is thrown in vibration, it will be communicated to the walls of the box, which in turn move the air in the box, and the result is a musical tone issuing from its mouth. In telegraphing this effect will be produced, when electrical impulses are sent through the wire of the magnet, at the rate of 128 times per second. If a much greater or less rate is sent through the magnet neither the bar or box will respond. If now we make another receiver in all respects like the one described, except that the bar and box are tuned to respond—say when 200 impulses per second are sent through it—it will be practically silent for the 128 series, but will speak out plainly for its own rate—200 per second. If the two rates of vibration or impulses are sent simultaneously, both boxes will speak, but each a different tone. If we stop the sending of one rate and not the other, the one receiver made to respond to that rate will stop sounding.

If at the sending end the transmitted notes are cut up into long

and short sounds, representing the Morse code, with a common telegraphic key, each set of signals will be heard on its respective receiver without interference with each other.

Still another form of analysing receiver is shown at fig. 6. This contains all the essential parts of the last-described instrument, without the resonance box, but has an additional attachment, by which a common sounder or recording instrument is worked by means of a local circuit. In this instrument the magnet is mounted upon an ordinary base, and is provided with a tuned reed or bar of definite pitch—the same as the last described receiver. On top of the free end of the bar is a platinum point, slightly concave, so as to form a small cup for oil. Resting on this cup is a platinum point attached to a light lever which is pivoted at the other end. This lever is provided with a friction slide, so that its balance can be

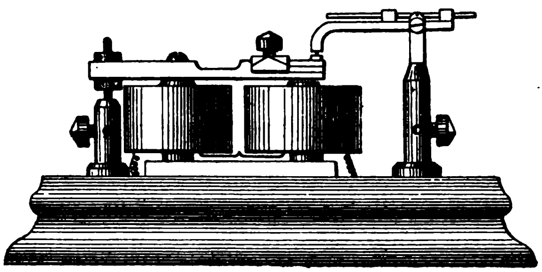


Fig. 6.

readily changed from light to heavy. This lever is so balanced that its natural rate of vibration is slower than that of the bar. As a consequence, when the bar is thrown in vibration the point of the lever is not able to follow, and so rattles or jars in the small cup. If a sounder and local battery are connected to it, so as to make the bar and lever a part of the circuit, each time the bar is thrown in vibration the circuit is practically opened, and the sounder lever falls on its back point. As soon as the vibration ceases the point stops jarring, the circuit is established, and the sounder closes. If the vibration is being interrupted at the sending end of the line by a Morse key, the effect on the sounder is the same as though a relay were in circuit instead of the analyser. This is probably the

most practical method of working the system, as the operator has nothing new to learn.

Thus far we have described the operation of the system in general, but there are many details of construction and methods of connecting batteries to line without which the system would be a failure for every-day telegraphic work. The proper construction of the vibrating bar or reed of the receiver is a very important point. It must be comparatively thick (unless suspended at both ends, as in fig. 4) at least one-fourth of an inch, and all the tuning done at one point near the fixed end. A thin bar, with a free end, will break into notes and respond more or less for any note, which would allow the signals to interfere. After the instruments had been properly made there still remained a problem, the solution of which furnished the key to the practical success of the system for telegraphic purposes. That problem was how to send the different tones into the line so that each set of electrical impulses would maintain its individuality, without which analysis would be impossible. In the first place, the wire must have the electrical vibrations thrown into it without opening the circuit or changing the resistance to any extent. Secondly, each tone must be sent from a separate battery power. This Mr. Gray has successfully accomplished.

Before describing the method of running the circuits we will glance at the transmitting instrument shown at fig. 7.

Fig. 7.

This instrument is precisely like the one described as fig. 1*a*. The only difference is it is separately mounted. It is kept in constant vibration by a local battery.

Fig. 8 shows a diagram view of two transmitters and two

receivers, with their connections. The local circuits, with their magnets, are left off to avoid confusion.

A and B represent two transmitters, placed at one end of a line; A′ and B′, two receivers at the other end. One end of the main battery is connected to line and the other end to ground. Each transmitter is placed in a shunt-wire, running from its main battery connections around one-half of the battery. A common open circuit key is placed in each of these shunt-wires. Suppose now the two reeds of A and B to be sounded, A making 264 vibrations per second, and B 320, just two tones or a major third above A. So long as the keys remain open the battery is all on the line steadily. If the key of transmitter A is closed, half of the battery is being thrown on and off the line at a rate of 264 times per second. This causes a succession of electrical waves to flow through the line at the same rate. If now the steel ribbon of the analyser A has been tuned in unison with these electrical waves it will respond and hum the same note as the transmitter; but if it is not in unison it will remain practically quiescent, so that the note can only be heard by submitting it to the most delicate test. To bring it in unison it is only necessary to turn the

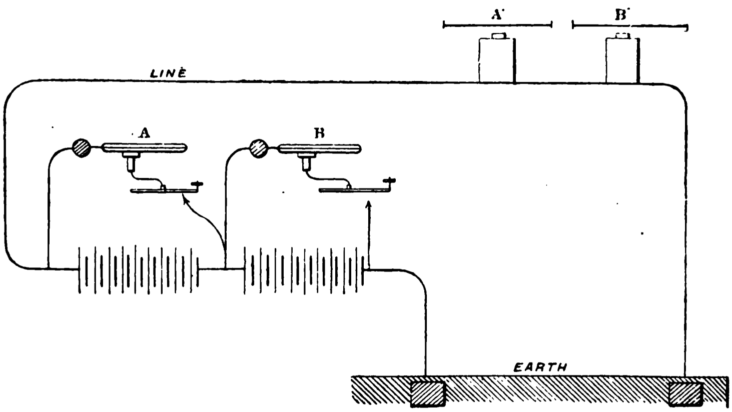


Fig. 8.

tuning-screw up or down, as the case may be. When the fundamental of the ribbon corresponds with that of the sending-reed, it

announces the fact by sounding out loud and full. If (having the key of transmitter A still closed, and, consequently, its corresponding analyser still sounding) we close the key belonging to transmitter B, the other half of the battery will be thrown on and off the line at the rate of 320 times per second, and another succession of electrical waves will flow through the line, this one being at the rate of 320 times per second. If the analyzer B is in proper tune, so that its fundamental is the same as that of its corresponding transmitter B, it will hum its note as long as the key is closed, making a chord with A′. In the same way a great number of different notes may be sounding at the same time at one end of a telegraphic line, and be heard simultaneously at the other end, each note sounding upon a different receiving instrument."

Of course, any of the three receivers described will operate in the same manner. The above diagram shows only two sets of apparatus, but the system admits of many more. Eight messages —four each way—have already been sent and received simultaneously through a single wire between New York and Philadelphia, for a number of days.

In working both ways, a main battery is used at both ends, and divided into as many sections as there are tones used. The receiver may be placed in a bridge or wound differentially, as in duplex.

There are many applications yet to be made of this system. Already Mr. Gray has applied it to a printing system which is at once rapid and reliable, and if in general use would greatly reduce the labour required to do a given amount of business, as well as the number of wires.—Extracted from "*The Telegrapher.*"

BELL'S ARTICULATING TELEPHONE.

Attempts have been made for many years past to transmit musical or articulate sounds to a distance by means of electrical communication, and some of the early experiments of the late Sir Charles Wheatstone were accompanied with so much success that it was hoped that a time would come when an instrument might be constructed not only to register graphically certain audible

sounds but to produce upon a diagram a set of signs by which the sounds of the human voice could be recorded; in other words, that it might become possible to construct an automatic reporter; and in the Loan Collection of scientific apparatus at South Kensington may be seen several instruments bearing upon these researches, and in which the vowel sounds are recorded by a series of distinctive curves.

In the year 1860, Philipp Reiss, of Friedrichsdorf, near Homburg, following the researches of Wertheim, Marian, and Henry upon the production of sounds by electricity, invented the telephone which bears his name, and which also may be seen at South Kensington. The telephone of Reis is of two parts; a transmitting instrument and a receiver. The former consists essentially of a stretched membrane, which, by vibrating in unison with the impulses it receives from musical sounds played near it, transforms those impulses into a series of electrical currents by a simple make-and-break arrangement, and these currents acting on the receiving instrument, which may be hundreds of miles distant, reproduce the corresponding notes, so that a tune played at one station can be distinctly heard at the other.

The receiving instrument is founded upon the well-known phenomenon discovered by Page in the year 1837, that a distinct sound accompanies the demagnetisation of an iron bar placed in an electro-magnetic helix. It consists of a soft iron bar about the size of a knitting needle, surrounded by a helix of wire which forms part of a voltaic circuit with the transmitting instrument, and for intensifying the effect both instruments are provided with sounding-boards, or resonators. From the above description it will be seen that if a note which makes (say) one hundred vibrations per second be sounded in the neighbourhood of the transmitting instrument, its membrane will make one hundred corresponding vibrations, making and breaking the voltaic current one hundred times, and producing one hundred demagnetisations in the receiving instrument for every second of time, so that exactly the same note that was sounded in the transmitter will be audible at the distant station. It is obvious that the duration of, and time between, two notes must be identical at both ends of the conducting wire, and thus is re-

produced automatically and without a possibility of error the elements which make up melody, viz , correctness of note combined with measure of time.

Following Reiss in Germany, Elisha Gray in America constructed in 1874 his far more perfect electric telephone, in which the transmitting instrument consists of a vibrating reed, which is at once a note-producer and a rheotome or contact-breaker. It is tuned like the reed of a harmonium to its proper note, and when adjusted can only transmit to the receiving instrument the number of currents per second corresponding to the vibrations producing its note. Elisha Gray's receiving instrument is electrically similar in principle to that of Reiss, but consists of a horse-shoe electro-magnet, mounted upon a wooden sounding-box or resonator, with a heavy armature attached to its poles. The transmitting instrument is provided with a key-board similar to that of a harmonium, and each note has its corresponding key and vibrating reed.

The same inventor has since introduced his splendidly worked out telephonic telegraph, by which four or more distinct messages may be transmitted in the Morse code simultaneously along a single wire. This apparatus depends for its principle upon having a vibrator at the receiving station, tuned so as to be affected only by its corresponding transmitter at the sending station, and thus the receiving instruments along a line of wire have the power of selecting those messages intended for themselves and letting all others pass. This has also been accomplished by a Danish engineer, M. Paul Lacour, who employs vibratory tuning-forks for transmitting the impulses, and a series of corresponding tuning-forks, each arm of which is inclosed in a magnetic helix for the selecting instrument. This selecting instrument can be used either as a receiving telephone, or by being employed as an intermediate relay may transmit the signals to ordinary telegraph instruments.

We give herewith illustrations of the transmitting and receiving instruments of Mr. Graham Bell's articulating telephone, by which the sound of the human voice may be transmitted by electricity along a telegraph line, and heard, as a voice, at the other end.

The articulating telephone of Mr. Graham Bell, like those of

Reiss and Gray, consists of two parts, a transmitting instrument and a receiver, and one cannot but be struck at the extreme simplicity of both instruments, so simple indeed that were it not for the high authority of Sir William Thomson one might be pardoned at entertaining some doubts of their capability of producing such marvellous results.

The transmitting instrument, which is represented in fig. 1, consists of a horizontal electro-magnet, attached to a pillar about 2 inches above a horizontal mahogany stand; in front of the poles

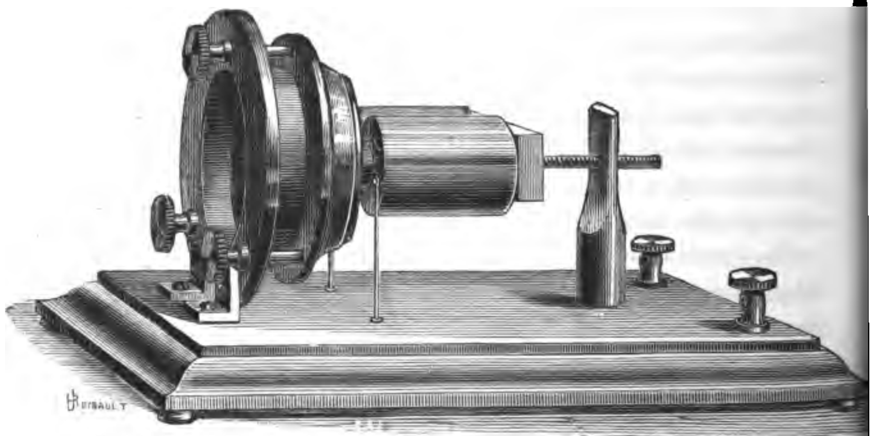


Fig. 1.

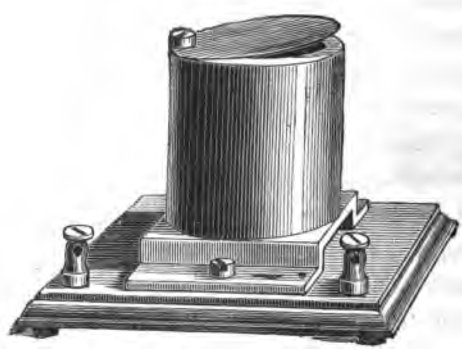


Fig. 2.

of this magnet—or, more correctly speaking, magneto electric inductor—is fixed to the stand in a vertical plane a circular brass

ring, over which is stretched a membrane, carrying at its centre a small oblong piece of soft iron, which plays in front of the inductor magnet whenever the membrane is in a state of vibration. This membrane can be tightened like a drum by the three mill-headed screws shown in the drawing. The ends of the coil surrounding the magnet terminate in two binding-screws, by which the instrument is put in circuit with the receiving instrument, which is shown in fig. 2. This instrument is nothing more than one of the tubular electro-magnets invented by M. Niclès in the year 1852, but which has been re-invented under various fancy names several times since. It consists of a vertical bar electro-magnet inclosed in a tube of soft iron, by which its magnetic field is condensed and its attractive power within that area increased. Over this is fixed, attached by a screw at a point near its circumference, a thin sheet iron armature of the thickness of a sheet of cartridge paper, and this when under the influence of the transmitted currents acts partly as a vibrator and partly as a resonator. The magnet with its armature is mounted upon a little bridge which is attached to a mahogany stand similar to that of the transmitting instrument.

The action of the apparatus is as follows: When a note or a word is sounded into the mouthpiece of the transmitter, its membrane vibrates in unison with the sound, and in doing so carries the soft iron inductor attached to it backwards and forwards in presence of the electro-magnet, inducing a series of magneto-electric currents in its surrounding helix, which are transmitted by the conducting wire to the receiving instrument, and a corresponding vibration is therefore set up in the thin iron armature sufficient to produce sonorous vibrations by which articulated words can be distinctly and clearly recognised.

In all previous attempts at producing this result the vibrations were produced by a make-and-break arrangement, so that while the number of vibrations per second as well as the time measures were correctly transmitted there was no variation in the strength of the current, whereby the quality of tone was also recorded. This defect did not prevent the transmission of pure musical notes, nor even the discord produced by a mixture of them, but the compli-

cated variations of tone, of quality, and of modulation, which make up the human voice, required something more than a mere isochronism of vibratory impulses.

In Mr. Bell's apparatus not only are the vibrations in the receiving instrument isochronous with those of the transmitting membrane, but they are at the same time similar in quality to the sound producing them, for, the currents being induced by an inductor vibrating with the voice, differences of amplitude of vibrations cause differences in strength of the impulses, and the articulate sound as of a person speaking is produced at the other end.

Of the capabilities of this very beautiful invention, we cannot give them better than in the words of an ear witness, and no less an authority than Sir William Thomson, who in his opening address to Section A at the British Association at Glasgow thus referred to it:

"In the Canadian Department I heard 'To be or not to be . . . there's the rub,' through an electric telegraph wire; but, scorning monosyllables, the electric articulation rose to higher flights, and gave me passages taken at random from the New York newspapers: 'S. S. Cox has arrived' (I failed to make out the 'S. S. Cox'); 'the City of New York;' 'Senator Morton;' 'the Senate has resolved to print a thousand extra copies;' 'the Americans in London have resolved to celebrate the coming 4th of July.' All this my own ears heard, spoken to me with unmistakable distinctness by the then circular disc armature of just such another little electro-magnet as this which I hold in my hand. The words were shouted with a clear and loud voice by my colleague judge, Professor Watson, at the far end of the telegraph wire, holding his mouth close to a stretched membrane, such as you see before you here, carrying a little piece of soft iron, which was thus made to perform in the neighbourhood of an electro-magnet, in circuit with the line, motions proportional to the sonorific motions of the air. This, the greatest by far of all the marvels of the electric telegraph, is due to a young countryman of our own, Mr. Graham Bell, of Edinburgh and Montreal and Boston, now becoming a naturalised citizen of the United States. Who can but admire the hardihood of invention which devised such very slight means to realise the

mathematical conception, that, if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of its current must vary continuously and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound."—*Engineering.*

THE TELEPHONE.*

By W. H. Preece, Memb. Inst.C.E., &c.

In the following paper I call instruments employed in the transmission of musical sounds, tone telephones, and those employed in the transmission of the human voice, articulating telephones.

In the year 1837, Page, an American physicist, discovered that the rapid magnetisation and demagnetisation of iron bars produced what he called "galvanic music." Musical notes depend upon the number of vibrations imparted to the air per second. If these exceed sixteen we obtain distinct notes. Hence, if the currents passing through an electro-magnet be made and broken more than sixteen times per second, we obtain "galvanic music" by the vibrations which the iron bar imparts to the air. The iron bar itself imparts these vibrations by its change of form each time it is magnetised or demagnetised.

De la Rive, of Geneva, in 1843, increased these musical effects by operating on long stretched wires which passed through open bobbins of insulated wire.

Philip Reiss, of Friedrichsdorf, in 1861, produced the first telephone which reproduced musical sounds at a distance. He utilised the discovery of Page by causing a vibrating diaphragm to rapidly make and break a galvanic circuit. The principle of his apparatus is shown in fig. 1 annexed, in which diagram *b* is a hollow wooden box into which the operator sings through the mouthpiece *a*. The sound of his voice throws the diaphragm *c* into rapid vibration, so as to make and break contact at the platinum

* Paper read before the British Association at Plymouth, August 1877.

points *d* at each vibration. This interrupts the current flowing from the batteries *e* as often as the diaphragm vibrates, and therefore magnetises and demagnetises the electro-magnet as often. Hence, whatever note be sounded into the box *a* the diaphragm *c* will vibrate to that note, and the electro-magnet *f* will similarly respond and therefore repeat that note.

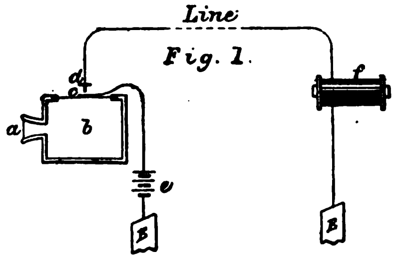


Fig. 1.

Musical sounds vary in tone, in intensity, and in quality. The tone depends on the number of vibrations per second only; the intensity on the amplitude or extent of those vibrations; the quality on the form of the waves made by the vibrating particles of air.

It is evident that in Reiss's telephone everything at the receiving end remains the same, excepting the number of vibrations, and therefore the sounds emitted by it varied only in tone, and were therefore notes and nothing more. The instrument remained a pretty philosophical toy and was of no practical value.

Cromwell Varley, in 1870, showed how sounds could be produced by rapidly charging and discharging a condenser.

Elisha Gray, of Chicago, in 1873, succeeded in producing tones from the fingers when rubbing a dry sonorous body, such as an ordinary tin can, whilst the intermittent currents sent by a vibrating tuning-fork were passing through it; and by attaching an electro-magnet to a hollow sounding-box, open at one end closed at the other, he was able to reproduce the tones of the musical notes transmitted. His electro-magnet had its armature rigidly fixed to one pole, and separated from the other by a space of $\frac{1}{64}$th

of an inch. He called it a resonator. The vibrations of this armature are imparted to the sounding-box and become therefore magnified. He constructed a keyboard, of two octaves compass, with steel reeds, each of which was tuned to its proper note and maintained in vibration by electro-magnets. When the key corresponding to any note was depressed its corresponding reed was connected to line, and the proper number of currents transmitted to the distant station, where they operated the resonator and thus reproduced the note. In this way tunes were played and the instrument became an electric organ. By attaching organ-pipes to his resonator he magnified the sounds and was able to fill a large hall with music played at places from 90 to 280 miles away. More than that he proved the practicability of transmitting chords and composite sounds to distant places. Gray also invented a method by which the intensity of the notes as well as their tones could be transmitted. Mr. Leonard Wray also introduced a capital receiver which emits the sounds received from a distance by means of Reiss's diaphragm.

It remained for Professor Graham Bell, of Boston, who has been working at this question with the true spirit of a philosopher since 1872, to make the discovery by which tone, intensity, and quality of sounds can all be sent. He has rendered it possible to reproduce the human voice with all its modulations at distant points. I have spoken with a person at various distances up to 32 miles; and through about a quarter of a mile I have heard Professor Bell breathe, laugh, sneeze, cough, and in fact make any sound the human voice can produce. Without explaining the various stages through which his apparatus has passed, it will be sufficient to explain it in its present form. Like Reiss, he throws a diaphragm into vibration, but Professor Bell's diaphragm is a disc of thin iron, *a*, which vibrates in front of a soft iron core *b*, attached to the pole of a permanent bar magnet N S (see fig. 2). This core becomes magnetised by the influence of the bar magnet N S, inducing all around it a magnetic field, and attracting the iron diaphragm towards it. Around this core is wound a small coil *c* of No. 38 silk-covered copper wire. One end of this wire is attached to the line wire, the other is connected to the earth. The apparatus at each

end is identically similar, so that it becomes alternately transmitter and receiver, first being put to the mouth to receive sounds and then to the ear to impart them. Now the operation of this apparatus depends upon the simple fact that any motion of the diaphragm *a* alters the condition of the magnet field surrounding the core *b*, and any alteration of the magnet field, that is, either its strengthening or weakening, means the induction of a current of electricity in the coil *c*. Moreover, the strength of this induced current depends upon the amplitude of the vibration, and its form on the rate of vibration. The number of currents sent of course

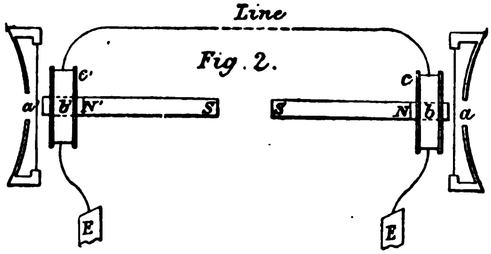


Fig. 2.

depends upon the number of vibrations of the diaphragm. Now each current induced in the coil *c* passes through the line-wire to the coil c^1, and then it alters the magnetisation of the core b^1, increasing or diminishing its attraction for the iron diaphragm a^1. Hence the diaphragm a^1 is vibrated also, and every vibration of the diaphragm *a* must be repeated on the diaphragm a^1 with a strength and form that must vary exactly together. Hence, whatever sound produces the vibration of *a* is repeated by a^1, because its vibrations are an exact repetition of those of *a*.

It is quite evident, however, that Bell's telephone is limited in its range. The currents operating it are very weak, and it is so sensitive to currents that when attached to a wire which passes in the neighbourhood of other wires it is subject to be acted upon by every current that passes through any one of those wires. Hence, on a busy line, it emits sounds that are very like the pattering of hail against a window, and which are so loud as to overpower the effects of the human voice.

Now, Mr. T. A. Edison, of New York, has endeavoured to remedy these defects in Bell's by introducing a transmitter, which

is operated by battery currents, whose strength are made to vary directly with the quality and intensity of the human voice. In carrying out his investigations in this field he has discovered the curious fact that the resistance of plumbago varies in some ratio inversely with the pressure brought to bear upon it. Starting from Reiss's transmitter he simply substitutes for the platinum point (*d*) a small cylinder of plumbago, and he finds that the resistance of this cylinder varies sufficiently with the pressure of the vibration of the diaphragm to cause the currents transmitted by it to vary in form and strength to reproduce all the varieties of the human voice. His receiver

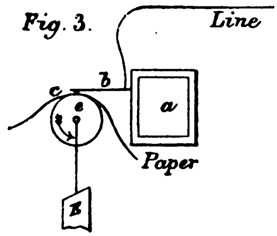


Fig. 3.

also is novel and peculiar. In 1874 he discovered that the friction between a platinum point and moist chemically prepared paper varied every time a current was passed between the two, so that the rate with which the paper moved was altered at will. Now, by attaching to a resonator *a* a spring *b*, whose platinum face *c* rested on the chemically prepared paper *d*, whenever the drum *e* was rotated and currents sent through the paper, the friction between *c* and *e* is so modified that vibrations are produced in the resonator *a*, and these vibrations are an exact reproduction of those given out by the transmitter at the other station.

Edison's telephone, though not in practical use in America, is under trial. In some experiments made with it songs and words were distinctly heard through 12,000 ohms, equal to a distance of 1000 miles of wire.

Bell's telephone is, however, in practical use in Boston, Providence, and New York. There are several private lines that use it in Boston, and several more are under construction. I tried two of them, and, though we succeeded in conversing, the result was not so satisfactory as experiment led one to anticipate. The

interferences of working wires will seriously retard the employment of his apparatus, but there is no doubt that scientific inquiry and patient skill will rapidly eliminate all practical defects.

To Professor Graham Bell must be accorded the full credit of being the first to transmit the human voice to distances beyond the reach of the ear and the eye by means of electric currents.

THE ELECTRICITY OF LEAVES.

At the last meeting of the Royal Society Dr. Burdon-Sanderson read a paper on the mechanical effects and the electrical disturbance consequent on excitation of the leaf of *Dionæa musci pula.* The mechanism by which the leaf of Dionæa closes has already been studied by Mr. Darwin, but the experiments now made add much to our knowledge of the nature of the excito-contractile process in plants and animals. The first set of experiments were to determine the time that elapsed between touching one of the sensitive hairs and the first perceptible motion. The touches were given at intervals of two minutes. The first half-dozen produced no mechanical effect. Then 25 successive touches produced effects which variously took 7, 5, and 3 seconds before they caused actual motion. The 26th touch produced motion in 2· seconds, and at the 27th the leaf closed. It was found by attaching a one-gramme weight that, with each touch after closing, the leaf clenched tighter. With regard to the electrical condition of the leaf it is found the external surface is positive to the internal. The electrical disturbance is strictly limited to the surface of the leaf, and does not extend to the petiole: the petiole simply serves as an ordinary moist conductor. Experiments were also made to ascertain the centre of greatest electrical intensity in the leaf, and tables giving the results of many experiments have been drawn up. In animals it has long been known that only the nervous and muscular tissues are electromotive, and it would appear that in plants it is the leaf alone that is electromotive. It was found by shifting the needle-

points to different parts of the leaf that when one part was exhausted and would produce no effects other parts would, thus indicating that the excitability of the plant is a property possessed independently by the protoplasm of every cell in the excitable area. Experiments have also been made as to the time that elapses between touching a hair and the manifestation of electric disturbance, and it is found to be from one-sixth to one-eighth of a second. Similar experiments on other plants are promised.

TALL CHIMNEYS AND ELECTRIC CONDUCTORS.

There are few chimneys which have any peculiar historic interest, but an exception is presented in one built at Glasgow by Mr. Joseph Townsend, and attached to that gentleman's chemical works. This chimney is to its neighbours what Mont Blanc is to the rest of the Alps—a giant among pigmies. The foundation of this chimney was laid in March, 1857, and on the 6th of October, 1859, the coping was added at the top, at a height of 468 feet from the foundation, and 454 feet from the level of the ground.

At the foundation the outside diameter is 50 feet, and at the surface it has diminished to 32 feet, while at the top of the coping the diameter is 12 feet 8 inches. On the 9th of September, 1859, and while the chimney was still unfinished, and therefore before the mortar was dry, a storm occurred, and resulted in swinging the chimney out of the perpendicular to the extent of 5 feet at the top. This accident, though perhaps directly due to the storm, had its origin in a neglect in the building process. Proper allowance had not been made for the contraction of the mortar used in setting the bricks, and as a consequence a certain number of planks were under a great pressure, being arched in the centre. Suddenly one of these at one side gave way in the oscillation caused by the storm, and with the unequal pressure the chimney was then forced from the perpendicular to the extent above stated. That the accident occurred in this way Mr. Joseph Townsend ascertained by

personal observation. For a time some fear was entertained that the whole chimney would come down, but on the 21st of the same month measures were taken to prevent this, and by the 1st of October the whole was restored to the original upright form. This was effected by sawing the chimney on the side nearest to an imaginary straight line. The following figures give the intervals at which cuttings were made:—

1	-	-	128 feet from the top		
2	-	-	49 feet below 1		
3	-	-	22	,,	2
4	-	-	15	,,	3
5	-	-	12	,,	4
6	-	-	19	,,	5
7	-	-	20	,,	6
8	-	-	13	,,	7
9	-	-	20	,,	8
10	-	-	30	,,	9
11	-	-	40	,,	10
12	-	-	40	,,	11
13	-	-	41	,,	12
			449 feet.		

When the chimney was only two years old it was struck by lightning, and a fire ensued, the composition gas-tubing being melted at a distance of 100 feet from the gas meter, though this latter was situated 20 feet from the chimney. To understand how this happened it is necessary to state a few additional facts. The chimney was provided with an electric conductor on one side, and a coil, which united with the conductor near the ground, where together they were bound to an iron rod and passed through a well of water, situated near the side of the foundation, 7 feet square and 2 feet deep, and thence down about 8 feet into the earth. Now, into this well comes the drainage of the works, and, further, the discharge pipe from a water-closet, and it was found on investigation, that, although the pipe actually discharging into the well was of stoneware, yet, further back, it was in connection

with one of cast iron. This latter pipe, being midway between the conductor and the gas composition tubing, must have served as a vehicle for the electricity, which must then have completed its circuit by the gas-pipe, which was thereby melted, and, the gas escaping, caused the fire.

To prevent the recurrence of such an accident the cast-iron pipe was removed and one of stone-ware substituted. All now went well till three years ago, when the chimney was again struck by lightning at 150 feet from the top, 30 bricks being then dashed out. Again an examination was instituted, and it was found that a separation had been effected between the conductor and the rod of iron with which it was bound where it passed through the well at the bottom. This separation had probably happened before the accident occurred and so possibly caused it. A new rod 10 feet long and passing 8 feet into the earth was now substituted for binding the conductor and coil together, and the whole was well tallowed to prevent oxidation, and was finally inclosed in a wooden box, of which the side of the chimney made the fourth. But a year ago the chimney was once more struck by lightning on the opposite side to that which was last attacked, that is, on the side along which descends the conducting rod. On this occasion a part of the coping stone was knocked off, and Mr. Joseph Townsend, impressed with the necessity of making some material change in the whole system of protection from lightning, is now providing the chimney with an apparatus which it is to be desired will fulfil its object.

This arrangement may be described in a few words. On the top of the coping stone are fixed four equidistant rods, about 3 inches wide and 1 inch thick; these terminate in stars or arrow-heads, and above them in the centre ascends a rod 20 feet long and higher than the rest, terminating in a double arrow-head. All these are properly connected with bands of iron, and are placed in good communication with the electric conductor and coils.

As may be readily imagined, there is some difficulty and not a little danger in raising such masses of iron to the height of 470 feet, but still more difficult and dangerous is it to construct the apparatus at the top, and fix it and bolt it together as is required.

For besides the exposure of the workman to the gases from the chimney, the atmosphere is often highly electric at that height, and freedom from sudden wind cannot be insured. The construction is nevertheless approaching completion, and the whole of it has been done by one man, Mr. R. Hall.

In concluding this sketch, which we hope may prove of some interest to manufacturers who have tall chimneys attached to their works, we would merely point out that not a little success of the working of an electric conductor depends upon the way in which it is sought to distribute the electric current over the earth. It is not sufficient simply to pass the rod down so many feet into the ground, but it should terminate preferably in a plate or sheet of iron so as to present a good surface for diffusion.—*Iron.*

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NEVETT, F. W.	Direct United States Cable Company, Torbay, U.S.
NEWBURY, GEORGE C. . . .	Postal Telegraphs, General Post Office, E.C.
NEWITT, C.	Anglo-American Telegraph Company, Hearts Content, Newfoundland.
NEWMAN, A. E.	River Plate Telegraph Company, Monte Video.

NEWNHAM, W. A.	Indo-European Government Telegraph, Jask.
NEWSAM, THOMAS	Eastern Telegraph Company, Aden.
NIND, S. L.	Newcastle-on-Tyne.
NOBLE, MARTIN	Postal Telegraphs, Lancaster

OAKSHOT, A.	Postal Telegraphs, Southampton.
OATWAY, ARTHUR	Postal Telegraphs, General Post Office, E.C.
OGG, F. WILLIAM	11, Barclay Terrace, Barclay Road, Leytonstone, Essex.
O'HAIRE, J. M.	Government Telegraphs, Cape of Good Hope.
OLDERSHAW, J., Sergeant R.E.	Ipswich.

PAGE, ALFRED S.	India Rubber Co., Silvertown.
PAGE, CHARLES J.	Eastern Extension Telegraph Company, Batavia.
PARKER, J. E.	Western and Brazilian Telegraph Company, Bahia, Brazil.
PARKHAM, JAMES	Mitcham.
PARMITER, ALBERT	Postal Telegraphs, Reading.
PARSONE, E. W.	175, Adelaide Road, N.W.
PARSONS, The Hon. RICHARD	10, Connaught Place, W.
PARTRIDGE, G. NOBLE	Postal Telegraphs, Exeter.
PATERSON, EDWARD J.	Bedford Court, Covent Garden.
PATTEN, FREDERICK A.	Indo-European Government Telegraphs, Kurrachee.
PAYN, JOHN J.	6, Rue Pavé D'Amour, Marseilles.
PAYTER, J. W.	Telegraph Department, Melbourne, Victoria.
PERRY, F.	Anglo-American Telegraph Company, Hearts Content.
PETERS, ALFRED	Western and Brazilian Telegraph Company, Bahia.
PETERSEN, K.	German Union Telegraph Company, 4, Werder Strasse, Berlin.
PETTERSON, OSCAR F.	Direct United States Cable Company, Torbay, U.S.
PHILLIPS, W. R.	Paumbaum, India.
PHILLIPS, J. C., Qt-Master Sergt. R.E.	Portsmouth.
PHILPOTT, JAMES J.	Postal Telegraphs, Stock Exchange, E.C.
PINCHIN, WILLIAM HENRY	Postal Telegraphs, Exeter.

POMEROY, HENRY . . .	Postal Telegraphs, Athlone.
PORTELLI, ROBERT . . .	Eastern Telegraph Company Malta.
PRICE, H. P. B.	Eastern Telegraph Company, Marseilles.
PROBERT, J.	Postal Telegraphs, Moorgate Street Buildings, E.C.
PUGET, Lieut.-Col. GRANVILLE W. .	34th Regiment, Carlisle.
QUILLEY, HENRY . . .	General Post Office.
RAMSAY, JOHN, Capt. R.E. . . .	School of Military Engineering, Chatham.
RAPE, L. W.	Anglo-American Telegraph Company, Brest.
REID, FRANK	Postal Telegraphs, Newcastle-on-Tyne.
REYNELL, C. H. . . .	Madeira.
RICH, H. R.	Indian Government Telegraphs, Calcutta.
RICHARDS, A., Sergeant R.E. . .	Telegraph Service, South Camp, Aldershot.
RICHARDSON, H. . . .	Postal Telegraphs, Douglas, Isle of Man.
RIEMENSCHNEIDER, FREDK. W. C. .	Great Northern Telegraph Company, 7, Great Winchester Street Buildings, E.C.
RISCH, GUSTAV	6, Brighton Villas, Cranfield Road, Brockley.
RITSO, F. C. G.	7, Lothbury, E.C.
ROBERTS, MARTIN F., F.C.S. . .	Postal Telegraphs, Gloucester Road, N.W.
ROBERTSON, J.	Postal Telegraphs, Bristol.
ROBERTSON, F. L. . .	West Coast of America Telegraph Company, 98, Cannon Street, E.C.
ROLLS, EDWARD T. . . .	L. and S. W. Railway, Exeter.
ROSS, JOHN, Quartermaster-Sergt. R.E.	Chatham.
ROSS, Walter O.	Anglo-American Telegraph Company, St. Pierre, Newfoundland.
ROSSE, LEWIS W. . . .	Anglo-American Telegraph Company, Brest.
ROWE, THOMAS	Postal Telegraphs, Manchester.
RUSHTON, WILLIAM S. . . .	General Post Office, E.C.
RUSSELL, FRANK . . .	Woodfield Road, Harrow Road, N.
SALE, M. T., Capt. R.E. . . .	Chatham.

SAUNDERSON, R. L.	Anglo-American Telegraph Company, Duxbury, U. S.
SAX, JULIUS	108, Great Russell Street, W.C.
SCHAEFER, LOUIS	
SCHINDLER, ARTHUR H.	Telegraph Department, Teheran, Persia.
SCOTLAND, JAMES	Anglo-American Telegraph Company, St. Pierre.
SHAW, Captain W.	102nd Fusiliers, Garrison Instructor, Aldershot.
SHEPHERD, F.	Postal Telegraphs, Brighton.
SIEMENS, ALEXANDER	12, Queen Anne's Gate, Westminster.
SIMMONS, CHARLES J.	106, Weddington Road, Kentish Town.
SIMPSON, GEORGE	Indian Government Telegraphs, Mannar, Ceylon.
SLATER, WILLIAM	Western and Brazilian Telegraph Company, Para, Brazil.
SMALLPIECE, WILLIAM D.	Warren House, Basingstoke, Hants.
SMITH, BENJAMIN	Eastern Telegraph Company, Malta.
SMITH, JOSEPH H.	Post Office, Selby.
SMITH, A. TOULMIN	Selborne Chambers, Chancery Lane.
SMYTHE, JOHN	Valentia, Ireland.
SPAGNOLETTI, HYLTON	4, Circus Street, Marylebone, W.
SPARROW, W.	Putney.
SPRATT, G. O.	Eastern Telegraph Company, Porthcurno, Penzance.
SQUIRES, FREDERICK	Anglo-American Telegraph Company, Brest.
SQUIRES, GEORGE	Anglo-American Telegraph Company, Brest.
STACEY, CHARLES	Eastern Telegraph Company, Aden.
STACEY, GEORGE B.	Eastern Telegraph Company, Bombay.
STARKE, HENRY	Government Telegraph Department, Brisbane, Queensland.
STEET, S. G., F.R.C.S.	130, King Henry's Road, Hampstead.
STELGES, J.	South Eastern Railway Telegraph Works, Tunbridge.
STERNE, LOUIS	9, Victoria Chambers, Westminster, S.W.
STEVENS, FREDERICK P.	Port Darwin, South Australia.
STEVENS, HENRY	Port Darwin, South Australia.
STEVENSON, ED. ALF.	Telegraph Construction and Maintenance Company, Enderby's Wharf, East Greenwich, S.E.
STEVENSON, GEORGE	Eastern Telegraph Company, Alexandria, Egypt.
STEWART, D.	Postal Telegraphs, Glasgow.
STOKES, CHARLES S.	7, North Terrace, Brompton, S.W.
STOKES, HENRY L. S.	7, North Terrace, Alexander Square, S.W.

STOKES, JOHN SCOTT . . .	R. and A. General's Office, G. P. O.
STONE, WILLIAM H. . . .	Japanese Government Telegraphs, Tokei.
STOTT, JAMES	353, Bridgman Street, Bolton.
STOUT, ROBERT	Postmaster, Lerwick, Shetland Isles.
STOUT, THOMAS . . .	Care of R. Stout, Dunedin, Otago, New Zealand.
SUDWORTH, S.	Messrs. Warden, Muirhead, & Clark's, 29, Regency Street, Westminster, S.W.
SWEMMER, B.	Government Telegraphs, King William Town, Cape of Good Hope.
SYMINGTON, ROBERT STEVENSON .	Telegraph Engineer, Glasgow.
TALBOT, FRANK F. . . .	Rosario, Argentine Republic.
TALBOT, JAMES E. . . .	The Post Office, Margate.
TANSLEY, WILLIAM . . .	Postal Telegraphs, Portarlington
TAYLOR, WILLIAM GRIGOR . .	Eastern Extension Telegraph Company, Sydney.
THEILER, RICHARD . . .	86, Canonbury Road, N.
THOMAS, G.	Brazilian Submarine Telegraph Company, Pernambuco.
THOMSON, EDWARD H. . .	Care of the Liverpool United Tramways and Omnibus Company, Limited, Pudsey street, Liverpool.
THOMPSON, F. O. . . .	Direct United States Cable Company.
THOMPSON, THOMAS . . .	Post Office Telegraphs, Ramsgate.
THORNTON, F.	Messrs. Warden, Muirhead, & Clark's, 29, Regency Street, S.W.
TICEHURST, F. G. . . .	Battle, Sussex.
TIDDY, WILLIAM N. . . .	4, George Street, Hanover Square, W.
TILLY, G. B.	Eastern Telegraph Company, Gibraltar.
TISLEY, S. C.	172, Brompton Road, S.W.
TOLMÉ, JULIAN H. M.I.C.E. . .	1, Victoria Street, Westminster, S.W.
TOPPING, F. W.	Direct United States Cable Company, Ballinskelligs.
TRANFIELD, F. T. . . .	Anglo-American Telegraph Company, Valentia.
TRENAM, EDWIN . . .	Postal Telegraphs, Leeds.
TRIPPE, C.	Anglo-American Telegraph Company, Hearts Content.
TROTT, J. G.	13, Dartmouth Terrace, Lewisham, S.E.
TRUMAN, CHARLES . . .	23, Old Burlington Street, W.
TUBB, ALBERT	Postal Telegraphs, Southampton.
TUCK, W.	Eastern Telegraph Company, Suez, Egypt.

TUFFIELD, T. S.	16, Anglesea Road, Woolwich.
TUNBRIDGE, W. T.	London and North Western Railway, Stafford.
TURNER, W., Sergt.-Major R.E.	Postal Telegraphs, Canterbury.
UREN, JOHN GEORGE	Penzance.
VENNDT, C. F.	Great Northern Telegraph Company, London.
VERNEY, Captain R.N.	Rhianva, Bangor, North Wales.
VYLE, SAMUEL	Postal Telegraphs, Glasgow.
WALKER, WILLIAM K.	Sealdah Station, Calcutta, E. B. Railway, India.
WALPOLE, WILLIAM BOWMAN	75, Patshull Road, Kentish Town, N.W.
WALTON, JOHN	Postal Telegraphs, Birmingham.
WARD, H. R. P.	1, Basingbourne Villas, Albion Road, Tunbridge Wells.
WARNER, R. A.	Cassilla, No. 777, Buenos Ayres.
WARREN WILLIAM	George Town, Tasmania.
WATERS, HERBERT M.	9, Park Terrace, Greenwich, S.E.
WATKIN, Capt. R.A.	Shoeburyness.
WATSON, C. M., Lieut. R.E.	War Office, Whitehall.
WATT, GEORGE W. M.	108, Ball's Pond Road, N.
WEATHERALL, T. E.	Telegraph Construction and Maintenance Company, Greenwich.
WEBB, E. M.	Telegraph Works, Silvertown.
WEBBER, T. B.	Telegraph Department, Great Western Railway Company, Plymouth.
WEBSTER, J. K.	Anglo-American Telegraph Company, Brest.
WEEDON, E.	Anglo-American Telegraph Company, Hearts Content.
WELLS, W. LEWIS	Submarine Cables Trust, 66, Old Broad Street, E.C.
WERDERMANN, RICHARD	4, Prince's Street, Stamford Street, S.E.
WEST, GEORGE	Eastern Telegraph Company, Alexandria.
WHITE, F. H.	Anglo-American Telegraph Company, St. Pierre.
WHITMORE, MORTIMER, Lieut. R.E.	Postal Telegraphs, Telegraph Street, E.C.

WIGAN, GORDON . . .	2, Brick Court, Temple, E.C.
WILDE, EDWIN	Postal Telegraphs, Leeds.
WILKINSON, HENRY D. . . .	Eastern Extension Telegraph Company, Singapore.
WILLIAMS, A. G. . . .	Anglo-American Telegraph Company, Hearts Content.
WILLIAMS, R. PRICE . . .	Great George Street, Westminster. S.W.
WILLMOT, JOSEPH . . .	Postal Telegraphs, General Post Office, E.C.
WILMOT, T. J.	Direct United States Cable Company, Rye Beach, U.S.
WINTER, CHARLES E. . . .	5A, Ladbroke Grove, Notting Hill, W.
WOOD, MAJOR ALEXANDER . .	Abbey Wood, Kent.
WOOD, CHARLES BARKER . .	Superintendent Direct United States Cable Company, Chester.
WOODCOCK, W.	Anglo-American Telegraph Company Hearts Content.
WOOLLEN, C. H. . . .	Postal Telegraphs, Exeter.
WRAY, LEONARD . . .	Woodend House, Walthamstow.
YEATES, HORATIO . . .	33, King Street, Covent Garden.

Total Number of Associates 445

STUDENTS.

BOYES, J.	Eastern Telegraph Company, 66, Old Broad Street, E.C.
COCHRANE, W. H.	St. Stephen's Chambers, Westminster, S.W.
DAVIES, GEORGE L.	Latimer Clark, Muirhead, and Co., 29, Regency Street, Westminster, S.W.
GATEHOUSE, THOMAS	374, Euston Road, N.W.
GEE, BASIL	22, Cambridge Street, Hyde Park.
HAYES, ALFRED	
HOOPER, SAMUEL	Beechwood, Clapham Common.
KENNELLY, A. E.	Eastern Telegraph Company, Porthcurno, Penzance.
KIRKMAN, JOHN P.	4, Thurlow Road, Hampstead.
LEWIS, ROBERT	Canada Villa, Cambridge Road, Anerley, Surrey.
MCKAIN, H. F.	
MEHRTENS, JOHN	
PALLISER, EDWARD	
PHILLIPS, C. H.	22, Cadogan Terrace, Victoria Park.
PENA, JOSÉ	Physical Laboratory, King's College, W.C.
WALROND, THEO. C. T.	5, Netherwood Road, West Kensington Park.
WARREN, J. D.	19, Pelham Street, South Kensington.

Total Number of Students . . . 17

TOTAL NUMBER OF MEMBERS.

Honorary Members	5
Foreign Members	149
Members	278
Associates	445
Students	17
Total	894